

Dutch Messengers:
A History of Science Publishing, 1930–1980

Library of the Written Word

VOLUME 7

The Industrial World

VOLUME 1

Dutch Messengers

A History of Science Publishing, 1930–1980

By

Cornelis D. Andriessse



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PREFACE

How was a small Dutch company able to grow into the world's largest science publishing house in just a few decades?

This book attempts to answer the question, not by economic analysis, but by examining the collaboration between the entrepreneurs and scientists who achieved it—the former as publishers, the latter as editors. For science, as one of the great social institutes, consists more than ever in the transmission of ideas and values. And also, when the success of an enterprise has to be explained, we do well to first ask whether this be due to the quality of its products.

Well then, we need know little of the history of Elsevier and North-Holland, two Amsterdam publishers who have joined forces and merged in 1970, to see that they put their scientific journals onto the market at the exact moment when there was a universal demand. This could only be the result of highly fruitful collaboration between competent editors—scientists who were researchers themselves and who had become famous for their discoveries—and far-sighted publishers, who were prepared to take considerable risks to capture a new market.

But this alone cannot explain their worldwide success. Professional knowledge and entrepreneurship are hardly to be found only in the Netherlands. Moreover, this country, small as it is, has played only a modest role in the rapid development of natural science (for by science here, we mean natural science), which for centuries has been the motor behind economic progress. So how otherwise do we explain this success? This book will attempt to show as well that it lay in the connection to a historical moment, and in the unique opportunity offered by this country's position.

It goes without saying that economic factors, and especially financial management, have contributed to the success of Elsevier and North-Holland. But a non-economic study of science publishing will delve deeper—certainly if we keep in sight its historical context—and will be essential in order to truly understand the essence of their development. Money can be made in many ways, but there is only one way to publish research results, namely with meticulous care, and a publisher's success lies in enticing scientists by the quality of his messages. It is the leaven in the process of creating science.

It makes little sense to write about science publishing without a discussion of the nature of science itself and, moreover, whoever wishes to write about a science publishing house cannot ignore scientific development. Science is the backbone of its history.

However, in books that have so far been published on the history of great houses like John Wiley & Sons, Butterworths, Blackwell, Saunders and McGraw-Hill, we read surprisingly little about the relationships with ‘their’ scientists.¹ Only in Heinz Sarkowski’s history of Springer-Verlag do we find significant attention given to the importance of these relationships, as well as insight into the effects of scientific progress on the development of the company. So what about Elsevier and North-Holland?

Astonishingly enough, no book has yet been published on the past of this science publishing house, the largest in the world. Not that no attempts have been made. In the 1970s Floris Bakels worked hard on its history, but had no opportunity to finish it and to have it published.² Only a summary was published in 1980, in an essay of 17 pages by the historian Willem van Leeuwen, as part of a collection of essays on science publishing in Europe.³ And in 2001 an independent publisher, Einar Fredriksson, added another 16-page essay.⁴

So why publish such an historical account now? That is to say: an historical account with due respect for the role of science, and due respect for the competitors during the half century, 1930 till 1980, in which the foundations for its success were laid. We do so because Anneke Frank-van Westrienen, a woman of great talent and herself

¹ Respectively: Moore, Jones, Norrington, Dusseau, and Burlingame—passim.

² Bakels’ unfinished typescript (with handwritten corrections and printed additions) is in Dutch and consists of three parts. The first part, *Elsevier 100*, counts 249 numbered pages and describes the history of Elsevier from 1880 to 1945. The second part, ‘Kroniek van Elseviers Wetenschappelijke Uitgeverij’ (*Chronicle of Elsevier’s Scientific Publishing Company*), counts 127 numbered pages and covers the years 1936–1956. [Letters and a shorter version that is partly overlapping with the first part, make clear that it has been thoroughly edited, probably by H.P.M. (Piet) Bergmans.] The third part is an *Appendix* of 54 unnumbered pages on the history of the 28 publishing companies, printing companies and bookshops that were acquired since 1970.

³ Meadows pp. 251–268 [‘The Decisive Years for International Science Publishing in the Netherlands after the Second World War’].

⁴ Fredriksson pp. 61–76 [‘The Dutch Publishing Scene: Elsevier and North-Holland’].

a historian, was loathe to accept that the history of North-Holland, the life work of her deceased husband, would never be told. Through a mutual acquaintance, Professor Henk van Bueren, she succeeded in convincing me that this was important history, and that time was pressing. Those who could still contribute their life stories were already old, and some of the main figures had died. Moreover, the rationality of a company—not the ravages of time—had caused many of the documents to disappear. In ten years time this piece of history, which had already suffered through the limits of oral tradition, would never be told.

I would like to thank the many who were prepared to share their experience and valuable insights with me: Konrad Akert, Marc Atkins, Pieter Bolman, Gerry Brown, Henk van Bueren, Robert Cahn, Marcel Dekker, Henk Edelman, Charles Ellis, Frans van Eysinga, Einar Fredriksson, Gordon Graham, Nico van Kampen, Wim Koops, Arie Manten, Yvonne Meijer-Praxmarer, Stefan Radt, Jacques Remarque, Per Saugman, Klaus Saur, Rob Schilperoort, Alexander Schimmelpenninck, Guus Schippers, Kai Siegbahn, Hans Trapman, Pierre Vinken, and Wim Wimmers.

I also obtained much helpful information from my correspondence with Dorien Daling, Cornelis de Jager, Arnoud de Kemp, Joost Kist, Jan Krips, Chris Morris, and Marietje N'Jie-van Rossem. I am especially grateful to Lex Lefebvre and Kees Thomassen who allowed me access to several unclassified archives in the Royal Library in The Hague.

I received my greatest support of all, however, from Anneke Frank-van Westrienen, Otto ter Haar, Paul Nijhoff Asser and Bart van Tongeren, and I thank them all for our many fruitful conversations together. Anneke Frank-van Westrienen gave me access to papers left by Daan Frank,⁵ and Otto ter Haar gave me the archive of Piet Bergmans, his

⁵ Frank's unpublished papers are in Dutch. They consist of an *Autobiography* of 4 pages (his sixty years until 1973) and three incomplete *Memoirs*. The first Memoir, marked 'K', has 40 pages and covers the years 1928–1944; 2 pages (numbers 29 and 42) seem to be missing. The second, marked *Fragments*, has 51 pages (numbered 43–94) and covers the years 1944–1962; it ends abruptly with 3 pages on the shares and regulations of North-Holland. The third, marked 'B', has 39 pages without number with text that is also used in *Fragments*, and 19 pages without number covering the years 1962–1970, with recollections of authors, colleagues and organizations, copies of letters and the draft of an address.

predecessor as director of Elsevier's Wetenschappelijke Uitgeverij,⁶ as well as the unfinished typescript of Floris Bakels.⁷

I would also like to express my appreciation to Derk Haank, the enthusiastic Reed Elsevier-director, who was prepared to reimburse my expenses with such alacrity, in spite of the small chance that 'his' company could be the publisher of my historical writing, and to all the staff at the Institute for the History and Foundations of Science in Utrecht, who extended me their hospitality for five years. Without their support and advice this book would never have been completed.

March 2008

Cornelis D. Andriessse

⁶ Bergmans' unpublished 'Persoonlijke Herinneringen' (*Personal Recollections*) count 15 numbered pages and cover the years 1946–1955. Bergmans' archive also contains letters from the period 1971–1975 in which the Elsevier-Nordemann-Interscience history is clarified.

⁷ See note 2 above.

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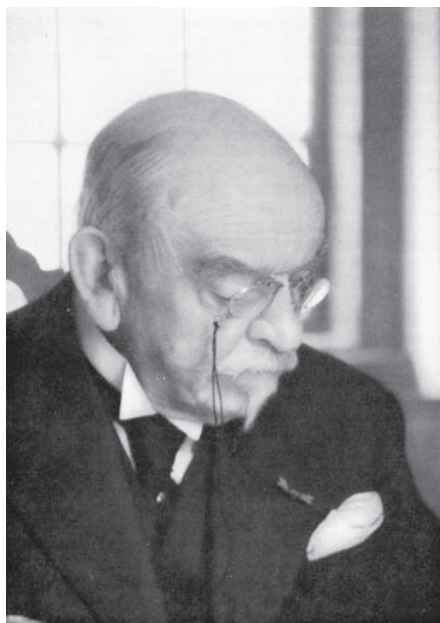


Fig. 1. W. (Wouter) Nijhoff.



Fig. 2. J.A. (Johan) Vollgraaf [*Œuvres Complètes de Christiaan Huygens*].



Fig. 3. W. (Wouter) Nijhoff Pzn.



Fig. 4. A.D. (Adriaan) Fokker [*Physica*, *Collected Papers of H.A. Lorentz*].



Fig. 5. J.P. (Ted) Klautz.



Fig. 6. F. (Fritz) Radt [*Encyclopedia of Organic Chemistry*].



Fig. 7. R.E.M. (Dolf) van den Brink.



Fig. 8. H.P.M. (Piet) Bergmans.



Fig. 9. W. (Willem) Gaade.



Fig. 10. H.G.K. (Hendrik) Westenbrink [*Biochimica et Biophysica Acta*].



Fig. 11. J. (Jacques) Remarque.

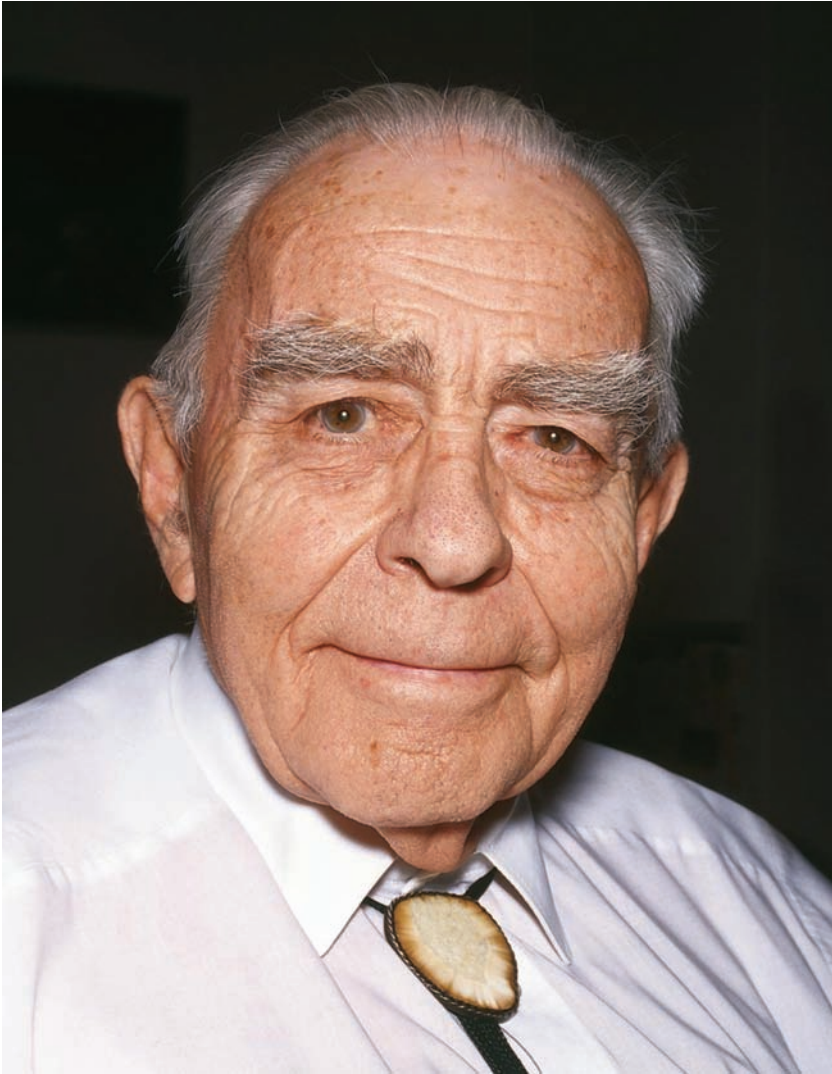


Fig. 12. K. (Konrad) Akert [*Brain Research*].

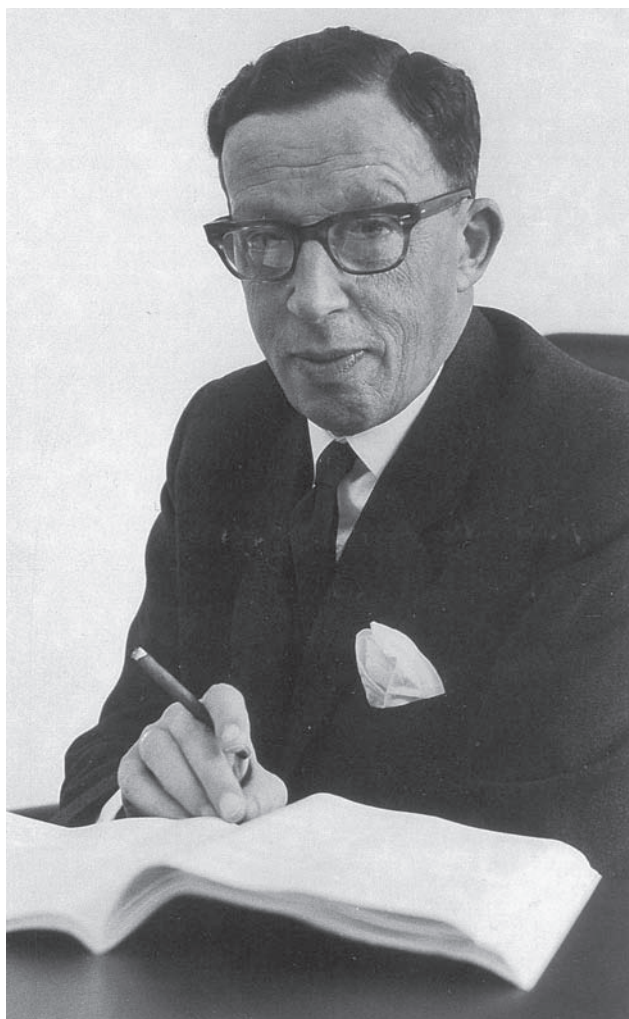


Fig. 13. M.D. (Daan) Frank.



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Fig. 16. G.E. (Gerry) Brown [*Physics Letters*].



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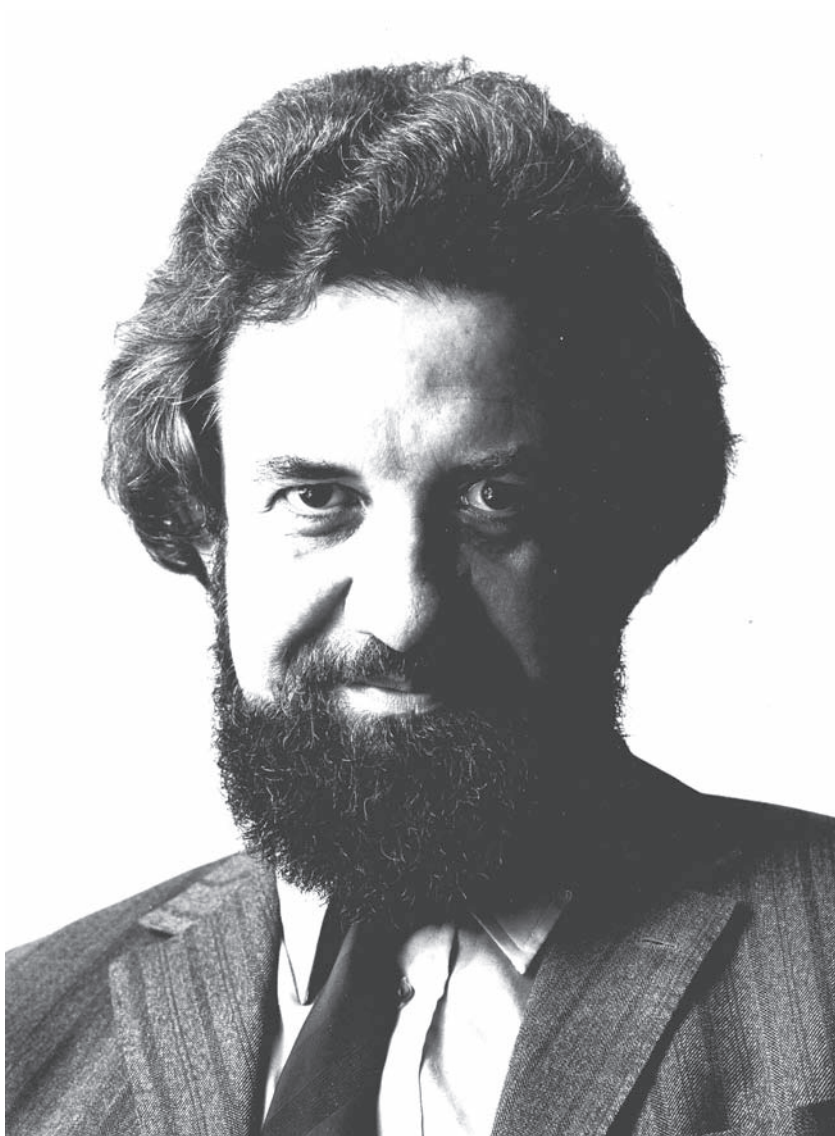


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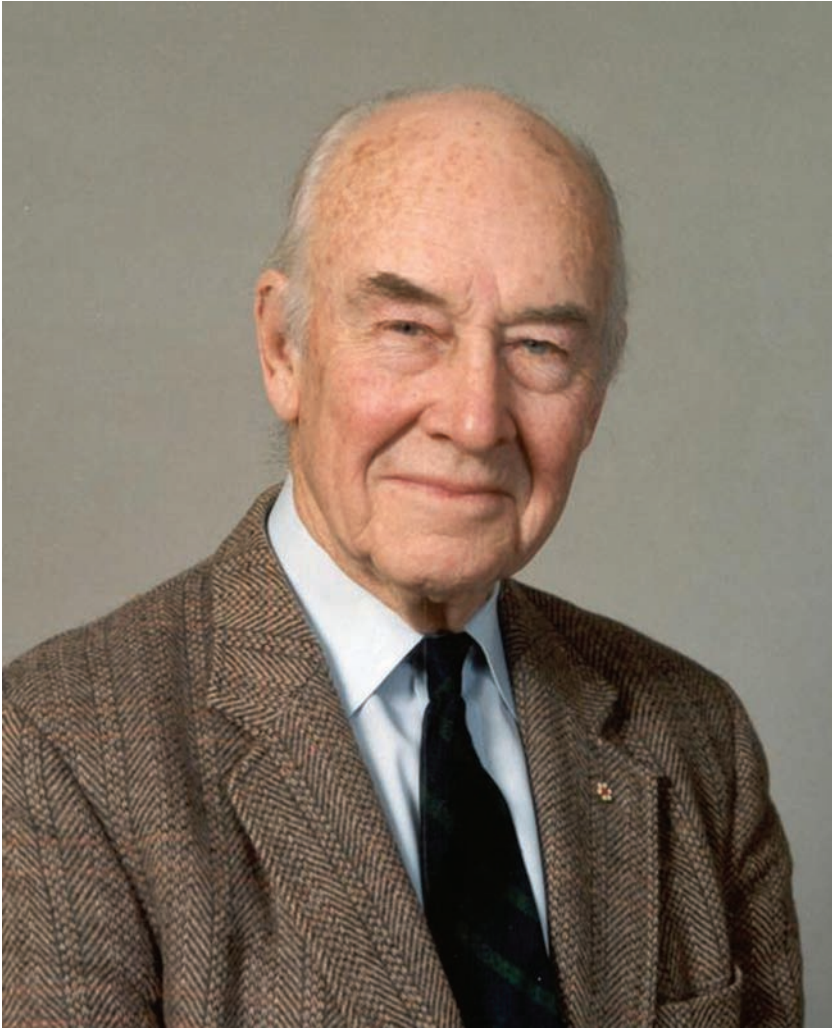


Fig. 30. J.T. (Tuzo) Wilson [*Tectonophysics*].



Fig. 31. P.J. (Pierre) Vinken [*Handbook of Clinical Neurology*].



Fig. 32. A. (Anton) Reidel (left) at a book fair in Cairo in 1971. The person on the right is J.F. (Jan) Hattink, later a publisher at Reidel.



Fig. 33. Jhr. F.W.B. (Frans) Humalda van Eysinga.



Fig. 34. R.A. (Rob) Schilperoort [*Plant Molecular Biology*].

CHAPTER ONE

ON SCIENCE PUBLISHING

*The phenomenon of human knowledge
is no doubt the greatest miracle in our universe*
(Karl Popper)

The universe is 13.7 billion years old. Let that be said straightaway. Not 13.6, or 13.8, but 13.7 billion years. So accurate is the message of the strange cold light that fills the cosmos and all of the outer space. It is so frail, however, that it can be registered only with extraterrestrial telescopes. Whoever wishes to know more may consult the September supplement of the *Astrophysical Journal* of 2003. But who does? The very idea that a final answer exists for the age of the world...

So we could try and keep this a secret. Why not? The man in the street would never know. He notices nothing of that cold light. People have other preoccupations, are unaware, and occupy themselves—if engaged in serious matters—with their mistakes. A bright Scot, William Thomson, once calculated the universe to be 98 million years old.¹ This was the age of the earth and the sun—about the heavens he remained silent. That was in 1863 and he published his finding in the *Philosophical Magazine*, which was circulated by the company Taylor & Francis. Couldn't we have left it at that? It might be four times as much or four times as little, what difference does it make? At any rate, it was unimaginably old and people grew weary of the question.

An enthusiastic Frenchman, Georges-Louis Buffon, had already come up earlier with proof that the world was older than we read in the Bible—no less than 75,000 years old.² But this was a thousand times shorter than was claimed a century later, and would have been forgotten if such discoveries were not written down and remembered. By publishing them our insight grows. It is through Man's thirst for logic that more and more is understood. Buffon's elegant and evocatively written *Histoire naturelle générale*, which rolled off the royal press

¹ Barrow & Tipler p. 160.

² Kors **1** pp. 176–180 (Lemma by P.R. Sloan).

in Paris in 1749, attracted lively interest and was translated into German, English, Italian, Dutch and Spanish. Of course, the theologians were at loggerheads with Buffon. It should never have been published, and definitely not with royal approval. We do not easily relinquish our view of the world, and certainly not those assumptions of which we are unaware.

Around fifty years ago, the historian Thomas Kuhn became interested in the significance of ‘fixed’ ways of seeing, and habits of thought—social-psychological paradigms, he called them.³ Philosophers of science thought that knowledge about nature came about by our alertness to unfamiliar phenomena, or else by keen observation, and then applying logical reasoning to determine their cause. Apparently this was not at all the case. On the contrary, much that was of interest appeared to be discovered by indiscriminately following rules that had proved their worth, and which fitted a paradigm. Only when this ‘normal’ manner of solving a problem failed, and the problem was particularly compelling, was the paradigm abandoned and the moment was ripe for a scientific revolution—a revolution that changed observation and logical reasoning forever.

We shall not dwell here upon the discussion that Kuhn set in motion after the publication of his *Structure of Scientific Revolutions* in 1962. For us this is mostly irrelevant. The most important conclusion was that ‘revolutionary’ science is not so easily distinguished from ‘normal’ science, and that empiricism and logic are always important, also in ‘normal’ science. Therefore, there is little point in wasting words on the sociologists who subsequently took themselves off to the laboratories to see how people obtained new knowledge.⁴ They labour under the misconception that truth is determined by power relations amongst researchers and not by nature, without being able to give a single example of scientific error caused by psychological, economic or political pressure that was not ultimately rectified. The truth, which time

³ Kuhn *passim*.

⁴ Latour & Woolgar *passim*; but see also Fruton pp. 24–26, where Latour is quoted from a later book: ‘It is because we have other interests and follow other roads that we find the myth of reason and of science to be unacceptable, intolerable, even immoral. We are no longer, alas, at the end of the 19th century, the most beautiful of the centuries, but at the end of the 20th, and the main source of pathology and mortality is reason itself, its pomps and its armaments.’

and again and according to a fixed pattern proclaims itself, triumphs over visions and poltergeists.

However, the social aspect of scientific undertaking presented by Kuhn is actually very significant, though not in the way that these sociologists think it to be. The ‘fixed’ ways of seeing and habits of thought in these laboratories and places of study are fundamental to the manner in which science comes into being. We do not speak here of some conventional wisdom to be taken with a pinch of salt, or some law to be taken on authority (δοξα), but of knowledge that is essentially certain (επιστημη). The point here is that science must be recognised by scientists as science. It must be universal, disinterested, original and sceptical—exchangeable for another that is better.⁵ It is established practice to publish discoveries after they have been tested and, if necessary, revised. This applies as much to ‘normal’ as to ‘revolutionary’ science, the only difference being that often the latter must wait long for recognition. It is the scientists themselves who have created a complicated and strictly controlled social structure to this end.

Here, in our history of scientific publishing, this structure is always implicitly present. This might seem obvious. However, at the time when Copernicus and Galileo published their revolutionary ideas, it did not exist. It emerged in the academies where the new science was being discussed, somewhat later, when the question arose as to how this could also be discussed and commented upon beyond the academy walls.

The scientific journal at the heart of this process was first published in the academies of Paris in 1665, even before the king had formally inaugurated the *Académie royale des sciences*.⁶ This *Journal des Sçavants* had three editors: Denis de Sallo de la Coudraye, Jean Gallois and Jean-Paul de la Rocque, and was to be a gigantic, exceptional undertaking, directed at all physicists, chemists, engineers and inventors in Europe. Quickly it became widely read, but just as quickly it became a source of discontent. The editors printed practically every article presented to them, and so became entangled in controversies about their content (some articles were even published—greatly against the editors’ will—in

⁵ Ziman pp. 28–55 (Chapter 3), with its exposition of CUDOS (Communalism, Universalism, Disinterestedness, Originality, and Scepticism).

⁶ Meadows pp. 1–22 (Chapter by A.A. Manten).

Amsterdam, in a pirate edition). As they were not responsible for the contents of these articles, they could only shrug their shoulders.

But Heinrich (Henry) Oldenburg, secretary of the Royal Society in London, was no man to shrug his shoulders. He, too, became involved in controversies, but found them tremendously interesting and saw them as grist for the mill for the *Philosophical Transactions*. This was the Royal Society journal, which he published four months after the first issue of the *Journal des Sçavants* appeared. From the start he made every effort to clarify controversies—that is, to discover the truth—in a long succession of diplomatic letters to Hooke, Huygens, Newton and other thin-skinned authors. Although he could not alter the text, he could suggest changes; also he could delay publication, or even refuse it altogether. In Oldenburg's letters we see already the emerging role of the scientific editor as we know it today.

Very soon we see something else as well: a third party becomes involved in reviewing a publication. Examples can be found everywhere. Already in 1682, Christiaan Huygens sought the opinion of a third party when he wrote from The Hague to Jean-Paul de la Rocque in Paris, 'I would be greatly obliged if this [my argument] be published in the *Journal des Sçavants*, so that those who are not acquainted with my proof do not think that [François] Catelan's remarks are of any significance. If he still returns to the matter, then you will oblige me to lay it before a professional, before it be published. Thus will even his honour also be well served.'⁷ Not only editors, but also authors came to appreciate the need for an expert. A knowledgeable person should be able to say whether an argument is valid, just as a notary could establish whether a transaction is in accordance with the law.

Over the years this 'notarial control', originally something incidental, grew through necessity to become the *peer review*. It could only be carried out by an equal and an old hand with enough experience to know that he was not the sole possessor of truth. The article would be passed on to him for approval, which entailed going through the calculations, verifying, where possible, the facts and checking the conclusions against other knowledge. He would then return it to the editor with a note of advice, on condition the author was not informed of his identity.⁸ The

⁷ Andriessse p. 320.

⁸ Ziman pp. 42–44: 'A referee's report is nominally personal, but it is really an individual interpretation of the communal criteria for 'good science'. Originality is only one of these criteria. As well as detecting errors, omissions, *non sequiturs* and other

fact that this process would also lead to professional opportunity with diverse *peers* per field of study was something that people discovered along the way. Driven by the same necessity, this control has prevailed in a distinctive style for journal articles.

While *Philosophical Transactions* still published articles on monsters, mermaids and other rare phenomena until well into the eighteenth century, from the start articles in the Parisian journal and the Leipzig journal (*Acta Eruditorum*, begun in 1682 and edited by Otto Mencke) were stricter and contained more mathematics.⁹ Only in 1755 did Jean le Rond d'Alembert, mathematician and editor of 'the' *Encyclopédie*, insist that mathematics was distinct from natural science, and that the two be separated.¹⁰ But the strict argumentation used for mathematics, found also in many physics publications, had developed its own impersonal style. Rhetoric and flowery language were no longer considered appropriate to *επιστημη*. The personal, with its many layers and ambiguity, interfered with rational argument and was replaced, in the eighteenth century, by the linear row with symbolic propositions. This meant that the role of formal logic and other forms of precise argumentation became excessive, even in the natural sciences.¹¹

Finally, scientific journals also offered something that discussion alone could seldom hope to do. With printing techniques, everything could be illustrated. (In fact, the basic technique has remained the same, ever since its invention by Johannes Gutenberg in 1440, and it remains to be seen whether modern electronic media will change it.) So then authors started to use pictures and diagrams to clarify what words could only partly convey, if at all. The journal became the stage for the great role of pattern recognition in geology, botany and anatomy, and in fact in the theory and practice of all scientific investigation.¹²

Before we go on to the role of the publisher, let us look briefly at the dynamics of this process. The journal confronted authors with the work of researchers who they would perhaps never meet, and brought them in touch with ideas that in their own direct circle they might never

technical defects, referees are expected to assess a paper in terms of the overall credibility and scientific significance of its findings.'

⁹ Kors **4** pp. 43–47 (Lemma by J.E. McClellan III).

¹⁰ Kors **1** pp. 38–40 (Lemma by D.F. Essar).

¹¹ Ziman pp. 109–113.

¹² Ziman p. 120.

get to hear about. And isn't exposure to the unfamiliar an essential requirement for new science?¹³ (Essential, but not sufficient; there must also be time and money, for example.) So the stimulus of these journals was enormous and the number of contributions increased faster than the number of authors. In turn, this increase stimulated the growth of even more journals.

This dynamic has proved so strong that nowadays the growth of scientific knowledge is measured by the increase in the number of scientific journals. While in 1700 there were only a couple, in 1800 we find dozens, in 1900 around a thousand, and now it could well be over the hundred thousand. But there has always been chaff amongst the wheat, now probably more than ever. At the most ten thousand would seem to be of real quality—having a serious *peer review*—and, remarkably, half of these are marketed by no more than ten publishers. Reed Elsevier (previously Elsevier) alone, by far the largest, publishes no less than 1900 journals.¹⁴ We do not need to be strictly accurate to show this development. The rise in these figures conceals the differentiation within the natural sciences or, rather, the splintering into countless sub-disciplines in physics, chemistry and biology.

However, it is unlikely that these figures say much about knowledge. More is by no means better. Until recently many young people went into science as a career. Competing for scarce positions, they published much more than required for 'highly promising' young researchers and, in their wish to excel, specialised in trash—in ephemeral subjects at institutes that referred only to themselves.¹⁵

Now for the role of the publisher—naturally it is his role to bring the message from the researcher to other researchers. He is a messenger. Let us first look at how he delivers his message, since this is simpler than the way in which he obtains it.

The former could even be regarded as perfectly simple, if we ignore the economic complexities of the real market. He 'just' brings his

¹³ Garvey pp. 1–39 (Chapter on the role of scientific communication); on p. 4 we read: 'The psychology of scientific communication emphasizes the interaction between the scientist and his environment (a major element of this environment being other scientists). To put it simply, in order to have progress in science we must have variation and selection in interpretation.'

¹⁴ Fredriksson pp. 304–307; the data of Reed Elsevier and Harcourt, Inc. are combined.

¹⁵ Brown, Pais & Pippard pp. 2017–2031 (Chapter 27 by Philip Anderson).

journal with its message to the market, where those interested—other researchers—browse around. In practice, this means the library of the institutes for scientific research that subscribe to that particular journal. These will never be less than a few hundred, seldom more than a couple of thousand, and within these limits dependent upon the significance of the journal. Readers may be surprised at the modest numbers, but the number of specialists who wish to read up on their own specialist fields of study can, for social reasons, only be fairly small (this is explained by Derek de Solla Price).¹⁶ Moreover, these institutes are small in number. So the publisher sends them out, quickly, so as to be topical, but not until the subscription, which he has managed to negotiate by spreading catalogues and trial issues at scientific congresses and exhibitions, has been paid for. During his campaign, of course, he has met competitors.

This is current practice. Historically, it all began with the publisher selling his text to a printer, and with colporteurs, who he sent to book merchants in other countries to negotiate sales contracts and to limit as far as possible taxes, deductions and other transaction costs. But funding the printing process (with typesetting, correction and binding) was already a costly process, and travel and distribution costs were high, so the publisher did not publish just anything. For however erudite a text, if there was no demand then there were no returns. Every scarce good might have its value, but not every discovery proved marketable, even if this was only because of many a researcher's reluctance to relinquish the ownership rights to some interesting theory or discovery, which the publisher needed in order to market it. Publication could, by definition, only be commercial, and this has been the case from the very beginning.

Bonaventura Elzevier (whose name the current Elsevier company has adopted) and Jordaan Luchtmans in seventeenth century Leiden,¹⁷ Prosper Marchand and Marc-Michel Rey in eighteenth century Amsterdam and, to name a few more of the same period, Benjamin Franklin, Christoph Friedrich Nicolai, and Charles-Joseph Pancoucke in Baltimore, Berlin, and Paris, respectively—all these great publishers from the past were merchants.¹⁸ Even the sixteenth century publishers

¹⁶ Price pp. 62–91 (Chapter on invisible colleges).

¹⁷ Ophuijsen pp. 8–28 (Chapter on The House of Luchtmans, 1683–1848); for Bonaventura Elzevier, see Chapter 4 of this book.

¹⁸ Kors **3** pp. 372–378 (Lemma by J.D. Popkin).

of Oxford and Cambridge universities, who originally only printed theological and philosophical books, were commercial.¹⁹ And, of course, the first popular scientific journals were also only published for profit, the *Journal d'observations sur la Physique, l'Histoire naturelle et sur les Arts et Métiers*, marketed in 1771 by Jean-Baptiste François Rozier,²⁰ and, not to be forgotten, the widely read *Nature* by Alexander and Daniel MacMillan established in 1869.²¹

How the message is passed on may be obvious, but often it is not clear just how the publisher lays his hands on it in the first place. This brings us to a complex question, which is also the main theme of this book. If we say that he picked it up ‘somewhere’ in the world of researchers, we have some grain of truth. But how does he actually get hold of it? Not because he has a nose for it, surely? He must also have ears. And perhaps money, as well. However, we have no economic models for this world at our disposal, whatever claims there may be. There is nothing in the strict sense to sell, nothing of value. In order to acquire what is of worth, the publisher will have to become, as it were, part of it.

Richard Courant is an instructive example.²² This mathematician, whose classic two volume book from 1924, *Methoden der mathematischen Physik* continued to be published by Springer-Verlag was forced to flee Germany. In 1941, in the turbulent world of New York he set up what is now the Courant Institute of Mathematical Sciences, and also managed its publications, including the English language editions of this work. Such double talent, for science and for doing business, is seldom to be found amongst researchers. They are able to take the step to become an editor, and sometimes do, but to become a publisher is often too big a step. Specialisation, which is required nowadays to make any real contribution to science, has only made the step even bigger. Plainly,

¹⁹ Sutcliffe pp xiii–xiv (On the printing in Oxford between 1478 and 1518); Black pp. 1–20 (On the first printing in Cambridge in 1584, and the restart of printing in Oxford in 1585).

²⁰ Kronick p. 107 (*Journal de Rozier*).

²¹ MacMillan & Co. was founded in 1843 in Cambridge by the brothers Alexander and Daniel MacMillan; since 1858 it is established in London and since 1869 also in New York; although it is an important publishing house, it will not play a role in this book, since it isn't specialized in scientific publications.

²² O'Connor, John J. & Robertson, Edmund F., “Richard Courant” in the MacTutor History of Mathematics archive (Wikipedia).

therefore, we look, not for the double-talented, but for doubles, for those couples—of editor and publisher—who took to working together.

Here are some of these couples: Adriaan Fokker & Wouter Nijhoff; Fritz Radt & Ted Klautz; Earnest Rodd & Willem Gaade; Léon Rosenfeld or Kai Siegbahn & Daan Frank; Gerald Brown & Wim Wimmers; Konrad Akert & Jacques Remarque; Prakash Datta & Bart van Tongeren; Michael Lederer & Marc Atkins; Cornelis de Jager & Anton Reidel; and Tuzo Wilson & Frans van Eysinga.

We could easily have named ten more. They belong to the science publishing world of the Netherlands, and enable us to understand its recent history. This, at least, is our thesis. The great quality of these couples must have been a cause of the undeniable success of Dutch publishing in the world market.

A cause—not *the* cause. No single history can be interpreted as a mono-causal process, propelled by the machinery of capital, or progress, or a prevailing spirit, or a clash of cultures. History is by nature contingent. This is how we have come to regard it; this is the paradigm of today. And because coincidence is always important and—yes—because history seems to depend upon a string of coincidences, it could have been different. But could the undeniable success that so fascinates us have just been pure coincidence? A whim?

It is not an unhistorical history that we seek to write, however—a history that serves only to prove a hypothesis and omits everything that has no apparent bearing upon that hypothesis. Since this is about people and the manner in which they work together, we must take ‘everything’ into consideration, their background, their development, their work and insights, their institutes and organisations, science and market, politics... and add to all this a power of imagination that can bring alive the past. It must be a synthesis of culture and history.

These are some introductory remarks about the main themes to be discussed in this book. Now for the actual content.

Before the 1930s, Dutch publishers were rare on the market—an international market—of books and journals on natural science. This is illustrated by the history of Martinus Nijhoff in The Hague, the only house at that time to enjoy international recognition. The second world war and the German occupation of the Netherlands, however, put an end to this, and afterwards their science publications were discontinued.

But the Nazi regime also caused damage to science publishing in Germany, great damage, and to publishing houses that were of world renown. This is described in a short impressionistic chapter that is crucial because of the opportunity which it afforded to the Dutch.

At the heart of the book are four lengthy chapters about the Amsterdam publishers, Elsevier and North-Holland. Already in the 1930s, Elsevier had stepped into the vacuum created by German self-destruction, publishing English versions of German titles. North-Holland quickly followed after the war with a list of English language publications. Of course, it was not the companies that established these lists, but *people*—within the organisational and financial frameworks of these companies. There were really only two: Willem Gaade and Daan Frank. They fully understood that they must search for their editors, not in a small country like the Netherlands, but in the wider Anglo-American world. Their breakthrough came after about ten years, with their exceptionally successful journals on biochemistry and nuclear physics, exactly the fields which twentieth century science has to thank for its two icons—the double helix (1953) and the atomic bomb (1945). Up until 1970 these two publishing houses operated separately from one another, in more or less separate markets. After this they joined forces in the Associated Scientific Publishers, although it has to be said that this company was actually in the hands of Elsevier. At that time they had together 88 journals, nowhere near the 1900 of today.

We have another short chapter at the end that deals with the return of Martinus Nijhoff in the 1970s as a science publisher, but this time as part of a group of smaller publishers and fed by borrowed capital and know-how of Elsevier staff. The final short chapter is on Elsevier amidst the international competition of the 1980s—a competition it would win in 1991. However, our story essentially ends in 1980, because in the take-over vicissitudes of that time our couples leave the scene.

CHAPTER TWO

THE PUBLISHER OF HUYGENS AND LORENTZ

Lost Glory. This was our original title for this chapter. ‘Lost glory’ would, indeed, have been beautifully apt for the founding work passed down to us from a golden era by the geniuses Christiaan Huygens (1629–1695) and Hendrik Lorentz (1853–1928). However, it would also suggest that the glory of the house of Martinus Nijhoff had already faded when it published these great works, which was certainly not the case. Martinus Nijhoff was at the time a flourishing concern of world stature. But after that the house did go into decline—such things happen, and we shall return to this later.

Let us begin in retrospect. Any history of science publishing in the Netherlands of the mid-twentieth century must necessarily do so. For although this small country had no clear autonomous culture, being part of a wider international Europe, it was nevertheless in search of something unique to itself, its ‘genius’. Around 1900 the cultural elites had been successful in the visual arts, literature, philosophy, psychology, historiography, and especially the natural sciences, but after the First World War, which had largely passed the country by, this inspiration seemed to have deserted it. So what happened to this ‘genius’?¹ In fact, in the years 1930–1980, there were some outstanding researchers in the natural sciences who also enjoyed international status—the astronomer Jan Oort, the physicists Hendrik Kramers and Frits Zernike, the chemists Johan Bijvoet and Hugo Kruyt, the biologists Albert Kluyver and Niko Tinbergen, and not to forget the physical chemist Peter Debye (although it must be said that Tinbergen and Debye did their best work abroad). These illustrious scientists were however treading on established paths.

This was very different from the discoveries of the years 1880–1910, when Jacobus van ’t Hoff, Johannes van der Waals, Hendrik Lorentz, Pieter Zeeman and Heike Kamerlingh Onnes astonished the world.²

¹ Bank & Buuren pp. 270–274.

² Berkel, Helden & Palm pp. 130–154; see also pp. 406–611 for short biographies of the Dutch scientists mentioned below.

Van 't Hoff showed us what determines the rate of chemical reactions, and what is meant by a chemical equilibrium; Van der Waals found that the behaviour of gases and liquids is determined by one and the same molecular force; Lorentz and Zeeman demonstrated that a magnetic field penetrates deep into atoms (in the electrical charges that are known to exist there and which Lorentz called electrons), and Kamerlingh Onnes that, in extreme cold, substances can be superconducting. Lorentz, especially, was something of a genius in that his keen analysis of electromagnetism led him to the discovery, in 1892, of a force that an electron must undergo when it moves through a magnetic field—later known as the Lorentz force. The following year, in 1893, in his penetrating analysis of electromagnetism, he also came across a motion effect in time—one that seemed, physically, to be absurd—the Lorentz contraction. This made him a direct precursor of Albert Einstein, who in 1905 was able to explain this phenomenon with his special relativity theory. Here Lorentz reminds us of Huygens, a direct precursor of another great genius, Isaac Newton.

For Christiaan Huygens we must go back even further, to the years 1650–1700, when he played a key role, together with Jan Swammerdam and Antoni van Leeuwenhoek (and perhaps even Baruch de Spinoza), in the scientific revolution that led to our current rational interpretation of nature. With Huygens we think immediately of his invention of the pendulum clock or his discovery of Saturn's rings, but more importantly, because of its fundamental nature, of his correct expression of the laws of collision and centrifugal force in 1656 and 1659.³ Newton immediately recognised the importance of Huygens' formula for centrifugal force and built on it further in his great work of 1687, on the relationship between forces (including gravity) and motion. During the course of the eighteenth century people came to forget, with publications by Newtonians—not in the least by Dutchmen such as Willem 's Gravesande and Petrus Musschenbroek—that these were based upon the discoveries of Huygens. For almost two centuries his manuscripts lay gathering dust in the library of Leiden University. Whatever their contribution to science, this had been made already. So it took a writer, Conrad Busken Huet, to bring Huygens to our attention once again—and with this the 'genius' of the nation—in *Het Land van Rembrandt*, an imposing cultural-historical study, written in 1882.

³ Andriesse *passim*.

This brings us to the publication of the Huygens' manuscripts, which came about thanks to the initiative of a 60-year-old professor of mathematics in Leiden, David Bierens de Haan.⁴ It is no coincidence that Bierens de Haan began editing Huygens' letters in the same year that *Het Land van Rembrand* was published. Johannes Bosscha, director of the Polytechnic College in Delft, quickly sprang to his aid.⁵ But very soon, all who were involved in the project realised that a comprehensive, reputable publication of all the manuscripts would take far longer than the six or seven years that were first estimated. Due to ill health Bosscha had resigned his post at the Polytechnic, and in 1885 accepted a less demanding post as secretary of the Holland Society of Arts and Sciences in Haarlem. However, his organisation of the Huygens publication, carried out during the last twenty years of his life, was his greatest achievement, his life's work in fact, and gradually he came to resemble an apostle. He began his speech on the 200th anniversary of the death of Huygens as follows:

Paying one's last respects to a friend is one of the greater griefs of life. In our eyes he is an image of noble seriousness, undisturbed by fleeting passion, an image of clarity, hardly touched by the commotion of life.⁶

An image, though, which said more about Bosscha than about Huygens. Right from the start it was obvious that Martinus Nijhoff in The Hague would publish the *Œuvres Complètes de Christiaan Huygens*. Since 1866 Nijhoff had published the 'Haarlem archives', those of the Holland Society as well as those of the Teyler museum—a very special and fruitful relationship to which we will return. While in the process of setting up his publishing company in 1853, he very soon built up an almost exclusive relationship with the Ministry in charge of the arts and sciences. He had a special typeface designed and paper impressed with the watermark 'Christiaan Huygens' for this prestigious publication, which was paid for indirectly by the state. As we have said, transcription and annotation turned out to be no sinecure, so the first volume did not come out until 1888. In 1905, after completion of the tenth and last volume of the correspondence, Diederik Korteweg, professor of

⁴ Berkel, Helden & Palm pp. 386–387.

⁵ Biografisch Woordenboek van Nederland **1** p. 79 [Johannes Bosscha, Lemma by J. Charité].

⁶ Andriessse p. xviii.

mathematics in Amsterdam, took over the editing.⁷ Korteweg was a key figure around the turn of the century in the practice of mathematics in the Netherlands, not only in its application to physics (in the so-called Korteweg-de Vries equation), but also in embroidering upon contributions from the Dutch Golden Age by men such as Frans van Schooten, Johannes Hudde, Hendrik van Heuraet, and Johann Bernoulli. For the Huygens manuscripts, he limited himself to the pure and applied mathematics, which required five volumes alone, and the vast number of figures and formulas posed a great challenge for Wouter Nijhoff, who by this time had succeeded his father, Martinus, as director.

In 1927 the physicist Johan Vollgraff took over the task of editing the remaining manuscripts, and this brings us finally to the period we cover in this book.⁸ It was to be a period of surprises. While Huygens' work in mathematics might have been brilliant, but not truly innovative, his work in physics turned out to be both brilliant *and* innovative. As can be seen in volume 16, published in 1929, Huygens discovered his formula for centrifugal force by using a rotating frame of reference as his point of departure, meaning a system that is accelerated. We are not certain, but we assume that Lorentz, Vollgraff's tutor, provided the clarifying explanation. The formula can only hold—and it does hold!—if all motions are relative, also those in circular orbits. Newton was convinced that every motion is absolute, but Huygens felt obliged to reject this hypothesis, thereby raising a problem that was only solved in 1915 by Einstein with his general theory of relativity. Lorentz was one of the few who immediately understood this theory. Volume 19 had more surprises in store, including Huygens' theory of light as a wave phenomenon, which raised discussion on the mathematical, and also physical, insight that must have underpinned the principle of wave propagation (the Huygens principle). It took six volumes to cover the physics manuscripts, which dealt not only with theories, but also with instruments. Then there was still sufficient material for a final 921-page volume, number 22, with miscellany, diaries and a summary of life events, which could only be only published in 1950.

Vollgraff came to resemble, even more than Bosscha, an apostle. His modest enthusiasm, like frozen champagne, spurred him on to

⁷ Biografisch Woordenboek van Nederland **4** pp. 266–267 [Diederik Johannes Korteweg, Lemma by D. van Dalen].

⁸ Biografisch Woordenboek van Nederland **2** p. 79 [Johan Adriaan Vollgraff, Lemma by H.A.M. Snelders]; English *éloge* by Dirk J. Struik in *Isis* **57** (1966) 84.

create a veritable study of Huygens, with his articles on all the new material that came to light in the manuscripts. In 1947, with the last volume still to be published, a first biography had already come out in England. Vollgraff was fifty when he began his editing work, but as a member of the committee supervising the publication of the *Œuvres*, he had had twenty years to prepare. The son of a professor of classical languages in Utrecht, with an older brother to succeed his father, he became a science historian, but not before first studying physics in Leiden, as well as a brief spell as professor in Ghent. With his mastery of scholarly seventeenth century Latin, and the stately French he used in the commentaries (*Avertissements*), he would have presented a somewhat daunting authority for a publisher. Daunting, at any rate, is his opinion (in volume 22) of those who would like to know more about Huygens' amorous adventures, or of the person who accompanied him on his travels after his meeting with Newton—was it a gentleman or a lady, the same one, perhaps, who offered her services to his brother? In those prudish years one was expected not to ask. And it was not only in the psychology that Vollgraff encountered difficulties in seeing the wood for the trees in his search for context for the Huygens' manuscripts. In the 1930s and after, it was no longer Wouter Nijhoff, the son of Martinus, who published the texts, but another Wouter, a nephew of the first Wouter. 'Great and mighty, with his huge stature'⁹ he enters our story, Wouter Nijhoff Pzn (Pzn = Paul's son). For now it is time to meet this publisher family who took on this exceptional publication.

We begin with the grandfather, the man who established himself in 1853 as an antiquarian bookseller and publisher in an upstairs room of a house on the Veerkade in The Hague, 'still smelling of paint and sawdust.'¹⁰ This Martinus Nijhoff (1826–1894) came from a family that had already been in the book trade for half a century. Not wishing to compete with Frederik Muller, the Amsterdam antiquarian bookseller whom he had served as apprentice, he left for The Hague, where he could expect to do business at the Ministries and the Royal Library. And, sure enough, there was plenty of interest in these quarters for the large consignment of books that he had purchased with the family capital at auctions in Belgium. There was a lively clientele, too, at his

⁹ Gerits p. 58.

¹⁰ Nijhoff (*Het Huis*) pp. 3–7 and 8.

own auctions, for which informative and, above all, fine catalogues were printed. Publications quickly followed, such as *De Nederlandsche Spectator* and the *Bijdragen voor Vaderlandsche Geschiedenis*, as soon as the Leiden professor, Robert Fruin, began to edit these contributions. He knew that his clients were wealthy liberal-minded citizens, who took great interest in 'the social question' and state decrees. This is why he saw little prospect in the publication of fiction. Nijhoff was to be a serious-minded publishing company, without being specialist, and also without any special affinity with the natural sciences. In 1879, after Martinus had moved three times to larger premises, he took his son-in-law into the business, and then in 1891, when he had a staff of eight, his son Wouter joined as well.

Wouter Nijhoff (1866–1947) was cut out to succeed his father.¹¹ After finishing his gymnasium studies, he entered into apprenticeships with book traders in Frankfurt, London and Paris, and during these years abroad it became abundantly clear that he had inherited his father's mercantile spirit. Who else could have perceived so quickly that America—raw materialistic America—would also be the country where there was good business to be done with intellectual merchandise? In 1901 he travelled there for the first time, offering his services to the universities. Recently established, these universities had only small libraries, and were in dire need of more books. He told them that he could purchase these in Europe and have them sent over. As we are primarily concerned here with his work as publisher, we need only mention that this particular activity, which necessitated extensive travelling and the building up of a wide network of clients, was exceptionally lucrative and provided the capital he needed for his publications. And although this book is about natural science publishing, in order to describe the man we cannot avoid mentioning two works that brought him great approbation—his *Nederlandsche bibliographie van 1500 tot 1540*, the first volume of which came out in 1923, and *L'art typographique dans les Pays-Bas pendant les années 1400–1550*, which was published in 1926.

People spoke of this Wouter, too, as 'the great, and mighty'.¹² In fact, he was rather short, with a clubfoot, and made pitiless demands on himself. Whereas his father, to stick to technical terms, was an entrepre-

¹¹ Nijhoff (*Het Huis*) pp. 7–13.

¹² 'Wouter Nijhoff 1891–1941, Toespraak gehouden door [address by] Wouter Nijhoff Pzn' (unpublished brochure about five decades history of the firm of his uncle) p. 1.

neur of octavo format, he was large folio. In 1910, and with work for around twenty staff, he moved into premises that he had had built on the Lange Voorhout (at number 9)—a superb location in The Hague, and a splendid building which would be declared a listed building ten years later. But he traveled constantly, because he did not yet trust his assistants with the acquisition of important, costly libraries offered at European auctions. He even went to Berlin in the middle of the Great War (the First World War) so that, when it was over, he would be able to supply institutes such as the University of Louvain, which had been badly damaged by German bombs. Wouter, too, was something of a bomb himself. Pater Bonaventura Kruitwagen, a Friar Minor who advised him in his publication of postincunables (the above-mentioned *Nederlandsche bibliographie 1500–1540*), was witness to such an ‘explosion’. The priest was reminded of a short circuit:

People had to get accustomed to his foreign, grandiose way of thinking, talking and doing business, before they could understand his language and ideas. Two, strongly contrasting people dwelt within Wouter Nijhoff, a phenomenon one often meets in men who are totally engrossed in their company or their work. The explanation is easy to find. When on the work floor they operate under high pressure in every fibre of their mind and body. If they come up against something beyond the limits of their profession, especially if it happens unexpectedly, then there is danger of a fuse blowing.¹³

We find no evidence, however, of any blowing of fuses in Wouter’s relations with Diederik Korteweg. Korteweg, who taught in school before he became professor, and even managed to keep in check the impossible Luitzen Brouwer—the mathematical genius who did his dissertation in 1907 under his tutorship—would, together with Jan Advocaat, have quietly resolved any problems that arose with Wouter’s publication of the Huygens’ manuscripts. We find the name Advocaat among those exceptional staff members who were appointed around 1920, together with ‘A. Krijgsman, specialist in journals and parliamentary proceedings, but also artistic manager.’¹⁴ What was so special about Anton Krijgsman, the artistic manager, was that he had been a barber. His workplace was up in the attic, where the archives and journals that he was supposed to keep up-to-date were kept. People reached him via a

¹³ F.J. Bonaventura Kruitwagen, ‘In Memoriam Wouter Nijhoff 1866–1947’ (unpublished brochure).

¹⁴ Brochure of note 12, p. 13.

dark back-staircase, and the moment they set foot on the first step a bell would be made to tinkle. The reason for the bell was to give the staff member, whose hair Krijgsman was stealthily cutting, the time to dive, white sheet and all, behind the cupboards. This piece of folklore was still around in 1950, when it was described by a young member of staff.¹⁵ It reminds us of lines of poetry—another peculiarity of this firm:

Een deurbel klinkt. Daar moet hij binnen zijn
 Er staat geschreven: scheren en haarsnijden.
 Het klein vertrek met kasten aan weerszij
 Lijkt door de sterke geur van allerlei
 Parfumerie-artikelen nog kleiner.
 Awater—ik moet zeggen, ik ben blij
 Dat ik hem zie, ik was hem bijna kwijt,—
 Zit in een mantel van gesteven lijnwaad.¹⁶

A doorbell rings. He has to be inside.
 It stands written: shaving and haircutting.
 The small room with cupboards either side
 And the strong smell of all sorts
 Of perfumery articles seems even smaller.
 Awater—I have to say, I'm so glad
 To see him, I almost lost him,—
 There he sits in a coat of starched linen.

These lines belong to Martinus ('Pon') Nijhoff, the son of Wouter 'The Mighty', who worked briefly for the firm, but soon realised that this was not his life's calling. After studying Law he became a well-known poet.¹⁷ The poem *Awater* dates from 1934. It is sober, strong and direct, a style not previously seen in Dutch poetry.¹⁸ This was of little use, however, to the father, who had even less use for the physics that a second son wished to study.

¹⁵ Gerits pp. 80–81.

¹⁶ Nijhoff (*Verzamelde Gedichten*) p. 218.

¹⁷ Biografisch Woordenboek van Nederland 2 pp. 406–408 [Martinus Nijhoff, Lemma by W.A. Ornée].

¹⁸ Kossmann pp. 676–679; 'The poetry of the prominent writers—A. Roland Holst, J.C. Bloem, Martinus Nijhoff—was only vaguely connected with the philosophical or political movements of the period. It did not try to find a solution to any but individual problems, it had no ambition to serve a specific purpose [...] The best poets achieved after long years of practice a conciseness and precision probably never equalled in Dutch literature.'

To ensure the family grip on his publishing firm, in 1917, the year ‘Pon’ began publishing poetry, Wouter employed a nephew, young Wouter Pzn, who we have already mentioned.¹⁹ It was a good move, for the young man would turn out to be willing and dedicated, and not somebody who thought first and foremost of himself. At twenty two he was almost as old as ‘Pon’ and, as opposed to the poet, the work appealed to him. His father, Paul, was a partner in a large firm of booksellers (Scheltema & Holkema), and it went without saying that he would join Martinus Nijhoff after completing his gymnasium and military service. The young Wouter was a tall fellow, who would go his own sweet way as long he could light up a cigarette, and very soon he felt perfectly happy amongst the cupboards and shelves full of old books at the Lange Voorhout in the Hague. Three years later the elder Wouter sent him to America to revive the client network from before the Great War, and taught him to make special catalogues (including number 518: *The Hollanders in America*, published for the 300th anniversary of the founding of New York (1926), which became renowned as a work of art).²⁰ There was no need to go to auctions any more, for that was all over after the war. While the elder Wouter, now almost sixty, regularly caught the train and visited booksellers in Belgium and France to purchase books for the antiquarian book trade (at home he travelled in a Studebaker, a splendid carriage), he left young Wouter free to decide what should be published. In 1928 he made him co-director, appointed three deputy managers, and departed to the East Indies for a year’s holiday in the tropics. Surely a just reward for this entrepreneur in large folio?

Quest for the Lingua Franca

Up until now the firm had been doing very well, especially in the United States, where university libraries welcomed their services. They could obtain all European books and journals as well as antiquarian ones, for which they were only too keen. Crates full of European specialist

¹⁹ Nijhoff (*Het Huis*) pp. 13–14 (on Wouter Nijhoff Pzn); Martinus—the poet—tells little about his cousin, despite the important role the latter played; also based on recollections of Paul Nijhoff Asser on 1 March 2006 (who could give the necessary supplement).

²⁰ Brochure of note 12, p. 17; also mentioned in Nijhoff (*Het Huis*) p. 12.

literature went to America from hundreds of publishers, but with only one bill.²¹ Moreover, in the 1920s the firm acquired a reputation for stocking all the publications of important international organisations. Trade in rare publications from elsewhere was increasing rapidly and the number of staff had risen to over sixty. Also, immediately after the Great War, the foundation of the journal *Genetica*, edited by the botanist Johan Lotsy, a reader in Leiden, and his student Marius Sirks, added still further to the international name of the firm. And there was plenty of interest in the latest, sixth edition of the *Van Dale* (a dictionary of the Dutch language) of 1924, as well as in the publication of works in zoology, art history, law and geography.

His publications were not to be ephemera, but of lasting value. He wanted no novelettes or translations of foreign modish literature on his list, even if it made a fortune. There were publishers who made more profit with just one novel than he did with all his publications in a whole year. This knowledge left Nijhoff unmoved.²²

Wouter Pzn said this in 1947, on the death of Uncle Wouter and, because he knew himself to be bound to this tradition, it perfectly describes him too. But in the 1930s it was not so simple to know what was valuable, or what would keep its value. And for a house like Nijhoff, which lived largely off its foreign trade, the economic crisis of 1929 came as a great blow.²³ In less than four years international trade lost two thirds of its volume and its value. While diverse currencies quickly lost their value, the Dutch government waited until 1936 before abandoning the gold standard and allowing the guilder to be devalued. Export became more and more difficult, and the lucrative trade with America came gradually to a standstill: Ten of the 66 members of staff had to be made redundant, and those who could stay had to accept a drop in salary. Wouter Pzn had a hard time of it, fighting not so much for himself as for these more fortunate ones who still remained. In these years he had to ensure that turnover came from inside the country, with publications that still paid in this impoverished market.

²¹ Brochure of note 12, pp. 6 (the first American transaction with Tice & Lynch of 1904), 11 (trade problems during the First World War), 16 (intensification of the trade in the 1920s), 20 (trade problems due to unfavourable exchange rates).

²² Wouter Nijhof Pzn, 'In Memoriam Wouter Nijhoff 1866–1947' (unpublished brochure).

²³ Brochure of note 12, pp. 19–22.

In the summer of 1928, he was paid a visit by Adriaan Fokker, a valuable contact as it turned out. But on this occasion the distinguished visitor, with his small dark beard, initially only asked him if he wanted to publish a booklet on wave mechanics.²⁴ He, Fokker, had tried that spring to explain wave mechanics—then a brand new subject—during a popular lecture in, of all places, the oldest museum of the Netherlands, the ‘Teyler’s Physics Collection’ in Haarlem, which was part of a collection of rarities which had belonged to an eighteenth-century merchant. These popular scientific lectures were part of Fokker’s job since being elected to succeed Lorentz on his death in the beginning of that year, not only as curator of the museum, but also as extraordinary professor of physics in Leiden. Lorentz was carried out to burial as a celebrity, with Albert Einstein, Paul Langevin, Ernest Rutherford and other great men walking behind the bier.²⁵ Many thousands of the inhabitants of Haarlem watched the long black procession of mourners pass slowly by, and the funeral can still be seen on film. Because of the prestige attached to the Teyler lectures, thanks to Lorentz, and since Nijhoff had already published the *Archives du Musée Teyler*, Wouter saw his chance. Having just been appointed co-director, and knowing the old Wouter to be far away in the East Indies, he did not hesitate to publish Fokker’s booklet that same year, in a sober but tasteful edition. The following year he published another booklet of Fokker’s, on acoustics, and in 1930 another on forces and motions. Apart from the years 1931 and 1934, he published a booklet by Fokker, of between 40 and 60 pages, each year right up until the war. These were the subjects: liquids, vibrations, radio-activity, metals, fibrous tissues, ultrasound, and magnetism.²⁶ But why not in 1931 and 1934?

²⁴ Interview of Nico van Kampen on 29 October 2002 (who had made acquaintance with Fokker when he was a student in Leiden): ‘Once he stood in front of a picture of Lorentz, and I noticed that his neat little beard was exactly Lorentz’s.’ In the interview Van Kampen also spoke about the equation that established Fokker’s name: Fokker published the equation [in 1914] with a reasonable justification and promised that a proof would be forthcoming. Three years later Planck wrote that no proof had yet appeared and that he would now supply it. Actually his proof was not much more than Fokker’s justification. In the meantime Fokker had been called to arms to defend his country against Germany.’ See Kampen pp. 88–92.

²⁵ G.L. de Haas-Lorentz (Ed.), *H.A. Lorentz—Impressions of his Life and Work*, North-Holland, Amsterdam (1957); see also Biografisch Woordenboek van Nederland **1** pp. 346–349 [Hendrik Antoon Lorentz, Lemma by H.A.M. Snelders].

²⁶ *Archives du Musée Teyler*, Fondscatalogus van Martinus Nijhoff (1953), p. 25.

To find an answer, we need to look at the character of Adriaan Fokker (1887–1972), which we have to do anyway as he is a central figure.²⁷ He and no-one else would shape the scientific publishing list of Nijhoff. He was a man who possessed so many talents that he had no idea what to do with them all. After failing to excel in any one of them, he got bogged down in a 31-tone music scale. After a promising beginning in physics at Leiden, Fokker was briefly in Zurich in 1913, where he worked under Einstein, after completing a dissertation under Lorentz. His articles on the fluctuating motion of irradiated molecules and on an incorrect theory of gravity were published in the authoritative *Annalen der Physik*, founded by Hermann Helmholtz and published by Springer-Verlag. Then, in 1914, he had the bad luck of being called up for military service, for although the Netherlands were able to stay out of the Great War, the entire border had to be guarded. Only after three years was he able to pursue the academic career to which he aspired. Paul Ehrenfest in Leiden, impressed with what Fokker had already produced, took him on as an assistant, arranging for him to make a study of polarisation and magnetisation in an electric current, a study that he was just able to complete before he fell ill. Another caesura of three years. Up until 1921 he remained high up in the mountains, in Swiss Arosa, ridding himself of tuberculosis.

Was it due to his isolation that Fokker missed out on the quantum discussion?²⁸ In any case, he was not seen in the early 1920s in Göttingen, Copenhagen or Munich, places where the crucial questions were being discussed. We ask because Ehrenfest and Hendrik Kramers, whom he knew well, were both important in this field, making substantial contributions to the body of thought on formulating a wave equation for quantum mechanics, which was presented by Erwin Schrödinger, and published in the *Annalen der Physik* of 1926. What a discovery! (It cannot be repeated often enough that in 1926 the correct description of the behaviour of the smallest particles was discovered. Without this discovery, the electronics that are the basis of the information and communication culture of today could never have been developed.) Schrödinger was the same age as Fokker—also born in 1887—but

²⁷ Biografisch Woordenboek van Nederland **3** pp. 174–175 [Adriaan Daniël Fokker, Lemma by H.A.M. Snelders].

²⁸ Kragh pp. 163–167; in these *Quantum Generations* Fokker's name is entirely absent.

unafraid of tackling a fundamental problem. So was Fokker rather a lightweight?

To ask the question is to answer it. Fokker's response to the general theory of relativity is characteristic. Einstein had already explained the ideas of this theory in 1916, as well as the required tensor calculation, in the *Annalen der Physik*, but this was heavy stuff. Whatever lighter diversion there was to be had lay in the geometry, something that especially appealed to Fokker, even if it was a strange geometry, a *chronogeometry*.²⁹ Fokker invented this word, *en passant*, guarding it jealously right into his old age, when it appeared in the title of the summary work that he had published by Pergamon. He found it important to invent new words for new discoveries by physicists, which is why he also wanted existing words to be properly applied. He acquired a certain fame as a critic of the sloppy language use that can be found in every dissertation. His talent for language undoubtedly kept him from mathematics. In the end it was Georges Lemaître who carried off first prize in 1927 (for the correct solution of Einstein's equations of general relativity as applied to cosmology, showing that the physical universe must expand),³⁰ while he was only able to solve something more simple (namely, how the centre of gravity of a system of two free particles should be defined in relativity theory). Fokker knew that it was not earth-shaking. He published it in the *Nederlandsch Tijdschrift voor Natuurkunde*, which was not read abroad. He had founded this journal in 1921, together with Balthasar van der Pol and Eduard Oosterhuis. He edited it practically on his own, wrote reviews for it, and now and then contributed an original article. It was a society journal, with a circulation of 200 copies, which was printed by Van de Garde in Zaltbommel, a firm that was owned by the Nijhoff family.

This brings us back again to Fokker, and his request in 1928 that Wouter Nijhoff Pzn publish his booklet on wave mechanics. Although we have been unable to find any letters referring to this meeting, we cannot believe that these two just talked about the publication of booklets on popular lectures. Most certainly they would also have discussed

²⁹ Fokker (1929 and 1965—*Chronogeometrical Introduction*) *passim*.

³⁰ Kragh pp. 349–350; equivalent to Nye pp. 527–528: 'It is, therefore, reasonable to credit Lemaître with the discovery [of the expansion of the universe], possibly the most important ever made in the history of cosmology.'

an international version of the *Nederlandsch Tijdschrift voor Natuurkunde*.³¹ (The Nijhoff archive, or what was left of it in the Royal Library after the demise of the firm, contains only one document from 1933, when everything had already been settled: the size, subscriptions, subsidies, offprints, editing, and the prominent role of the Philips electronics concern.) The fact that Fokker, as we have said, did not manage a Teyler lecture in 1931, could be an indication of the difficulty he would have encountered in obtaining the support of physics professors in the Netherlands for his plan. Why would it be to their advantage to publish not in foreign journals but in *Physica*? Who would read *Physica*—the name that Fokker suggested for the journal? What would it cost? They already had their financial difficulties. They had seen great discoveries, and contributed to them, but their mood was pessimistic. When the economic crisis broke out, their work was the first to suffer cutbacks. One of them already spoke about:

the most drastic alienation of the natural sciences, of which a large part of Dutch intellectuals suffer; for example, many of our leaders, who in general come from legal circles and are unfamiliar with the role that these sciences play in modern society.³²

And, besides, there was the problem of language. The world community of academics had not spoken the same language for a long time—there was no longer a *lingua franca*. *Physica* was Latin, and everybody understood it, but otherwise Dutch physicists addressed their colleagues abroad in French, English or German. French had been the language of Huygens (when he did not use Latin) and also of Lorentz (in his most important publications), but for the Dutch it was a ‘foreign’ language. For a long time, and certainly since the second half of the nineteenth century, there had been a preference for English and German, and it was a close tie between these two ‘related’ languages as to which was used the most. In practical terms, it was a matter of where they could best contribute, to work in England (or America), or in Germany. Actually, it came down to the tension that existed between the Netherlands that was engaged with the Atlantic and the Netherlands that was engaged with the Continent.

³¹ The importance of the foundation of *Physica* for Martinus Nijhoff is underlined by the fact that Martinus, the poet, mentions it in *Het Huis* on p. 11.

³² Berkel, Helden & Palm p. 208.

It was plain for all to see: *Physica* would have to publish English as well as German articles, while not excluding French ones, so as to maintain good relations with Belgium. His flair for language and multilingualism made Fokker an ideal editor who, ever friendly and diplomatic, always managed to obtain articles from all the physics professors in both the Netherlands and Belgium. We can therefore easily attribute his not being able to manage the Teyler lecture again in 1934 to the time and effort he put into making the first issue a success. Now and again he was able to obtain articles from Germany and Switzerland, from Wolfgang Pauli, for example, but he wanted great authors, from whom he could accept ‘everything’.³³ Only once, in the 1950s, did he refuse an article. And what a commotion that caused! His perseverance and drive were remarkable. During the course of 1933—a shocking year altogether, and marked by the suicide of Ehrenfest, a great man who could be succeeded only by the brilliant Kramers—Fokker must have succeeded in getting sufficient promises for copy for the first year of publication. For Nijhoff, the publication of a journal of physics in three languages was an unprecedented adventure, and would be entirely at his own risk and his own expense. Charles Gilhuys, his accountant, would certainly have advised him against it. Fred Verhoef and Chiel Priem, in charge of production, had to cope not only with formulae but also with language problems. We shall meet Verhoef later when we come to content, and he becomes Fokker’s discussion partner. But whatever Wouter Nijhoff Pzn may have thought of it all, he would certainly have approved of a message that went in the face of malaise and pessimism.

At the end of 1933 Martinus Nijhoff announced that in the following year they would publish a multilingual journal on physics on the international market. Springer-Verlag took note!³⁴ People in Berlin quickly started to wonder whether, with *Physica*, the Dutch might not pose a threat to their global position in scientific journals. It might

³³ Interview of Nico van Kampen on 29 October 2002: ‘Fokker accepted everything that was submitted by colleague professors in the Netherlands and their students. He knew them well and he was convinced that the standard of their work was high. Would it have made sense to ask Kronig as referee for a paper by Kramers, and vice versa? The only exception I know of was Fokker’s refusal to publish an incomprehensible paper by H.J. Groenewold. Fokker was a man of status, as any editor should be, quite independent on the advice of experts. The high standard of *Physica*, at least until the 1970s, reflected the high quality of Dutch physics.’

³⁴ Sarkowski p. 331 (on *Physica*) and p. 354 (on *Zeitschrift für Physik*).

bode ill, not so much for the *Annalen der Physik* as for the *Zeitschrift für Physik*. Under the editorship of Karl Scheel and Hans Geiger, the *Zeitschrift für Physik* had expanded, in the 1920s, to become the leading international journal for modern physics, with 5000 pages per year. It was in German, but almost half of the articles were from foreign physicists in Western Europe and America. The first issue of *Physica*, which came out in the new year, suggested that ‘more than half’ the articles would be in German. It would become less than half, but the threat appeared to be real. A year later, in 1935, the share of Western articles in the *Zeitschrift für Physik* had fallen to one tenth. Of course, the departure of authors was due to political reasons—reasons that we shall deal with in the next chapter—but there was no evidence of this in the development of *Physica*. In the years 1934, 1935 and 1936 its number of pages (1207, 1120 and 1171) and number of articles (151, 144 and 144) hardly changed, the only shift being in language use (respectively 56%, 58% and 68% English; 38%, 35% and 25% German; 6%, 7% and 7% French). This growing preference for English would continue. In 1939, on the eve of the new war, 85% would be in English, 10% in German, and 5% in French, with practically the same-sized journal (1152 pages and 130 articles). The number of subscriptions to *Physica* during these years would have been around 500.

After launching *Physica*, the language issue arose in another form, when Fokker wished to create a monument in memory of his greatly esteemed tutor. So he persuaded Nijhoff to publish the *Collected Papers of H.A. Lorentz*.³⁵ Between 1935 and 1939, the young Wouter would publish the nine volumes this required, fully trusting that they were of great value. Hadn’t Pieter Zeeman, with whom Hendrik Lorentz had shared the Nobel Prize in 1902, collected precious material for it? But the publication turned out to be problematic. With its English title, Fokker addressed the Atlantic world, while much of Lorentz’ work was in French, and his dissertation, important as the first comprehensible exposition of Maxwell’s theory of electromagnetism, was even in Dutch. Shouldn’t the *Collected Papers*, therefore, have been presented in an English translation, as well as in the original language? Apparently this had been the plan, but Fokker begrudged the time required. A translation was given only of the dissertation, but this was into... French! Only

³⁵ Lorentz *passim*.

the forewords were in English, where he briefly indicated what was dealt with. But nowhere did he give any explanation. In the 1930s, when Lorentz' work was still regarded as difficult, this was certainly called for!

An example from the second volume: detective work is needed to see that the Lorentz-force is given by $\int \rho(\eta\gamma - \zeta\beta)d\tau$ and two other similar formulas on page 238. It is not revealed what the Greek letters represent here. Only on turning back do we see that β and γ are the strengths of a magnetic field in the y- and z-direction, ζ and η the speeds with which a charged particle (the electron) moves in these directions, and $\int \rho d\tau$ the charge of that particle. Those who manage to recognize in $(\eta\gamma - \zeta\beta)$ the result of a vector product in the x-direction, assuming that the x-, y- and z-directions are perpendicular to one another, will then recognise the Lorentz-force. Nowadays we usually write this in vector form. What is a scientific historian to make of a publication like this, without annotation?

It is surprising that such an experienced publisher as young Wouter Nijhoff could not persuade Fokker to add any biographical information about Lorentz to the *Collected Papers*. This, too, would have been welcome. It could have led to ideas. Indeed, a whole lifespan has passed, and still no-one has come up with a *Life & Works* of this great mind, who was as astute as he was gentle and modest. All in all, Fokker's edifice was not as edifying as an edifice should be. And Nijhoff would not have earned a penny out of it. After the war, when Fokker asked Wouter to publish the collected works of Kramers, the answer was no: nobody should approach him again with such publications...³⁶

During the Second World War, when the Netherlands was occupied by the Germans, the language question returned yet again. Once more it concerns *Physica*. In 1943 Fokker was put to the test. He had submitted to the *Wehrmachtsbefehlshaber* two articles by Philips staff members,

³⁶ Interview of Nico van Kampen on 29 October 2002: 'Fokker told the editorial committee of which I was member that Nijhoff wished to publish the *Collected Scientific Papers* of Kramers, since he had also published those of Lorentz. But he lost his interest when it became clear that the type-setting of the many formulas in this book would be very expensive, while a cheaper edition based on photocopies of the original papers was not according to his standing. Then we went to North-Holland, where Frank was ready to do so. The book appeared in 1956. From then on I had fruitful contacts with North-Holland, which published two of my books [those on plasma physics and on stochastic processes]'.

Johan Jonker and Bernard Tellegen, and the question was whether they were publishable, since military interest could be a reason to suppress them.³⁷ But not the possible military interest of these articles seemed to be a problem, it was their language. This was not so strange, because the Germans had not only assumed military authority, but also civil authority. So, the question became: could “*The positive grid current in electron valves*” be published, because it was in English? What the Germans meant, exactly, was unclear. Lack of clarity as to their real intentions was a Nazi tool to ‘rectify’ their occupied neighbours. Verhoef, at Nijhoff, thought that the content was not a problem and that the requirement was only to use German, and not English. But Fokker wrote back to him that the ban on the use of English was serious, and that the article could not be published. It would surely not have been because Fokker had given in; on the other hand very soon after

³⁷ Letters on the German intervention in the publication of *Physica*: On 3 October 1940, Fokker writes Nijhoff’s employee V. Funke about a possible censure of papers of military-technical significance, to which Funke answers that ‘this is nonsense’. Yet on 13 February 1941, Fokker writes Funke about two papers by P.J. Bouma that are being scrutinized by the Wehrmachtbefehlshaber Bruns and may be forbidden [and as it turns out, only one of these two is published, shortly after August 1942]. Much later, on 25 February 1943, Fokker writes Funke about German objections against the publication of ‘The positive grid current in electron valves’, a paper by Jonker and Tellegen. Funke answers that there is only question of a German request, not of a prohibition: he wants to publish. Fokker answers immediately (1 March) that the request is serious and that the paper cannot appear. Then there is a letter of 20 July 1943 from E.G. Verhoef to Fokker (Funke’s name will not return in the *Physica* correspondence) that by order of the German Reichskommissar the size of *Physica* has to be reduced to 40%: ‘Shall we stop?’ Fokker answers (17 August) that more can be printed on the still available paper. On 13 September 1943, Verhoef writes Fokker about the recognition of *Physica* by the Germans as a scientific journal, which implies restrictions on the use of English. In his reaction (1 October), Fokker announces that he will write a letter of protest to Mr J.K. van der Haagen of the OWK-Ministry on the ‘unlawful’ intervention in the language of *Physica*. On 12 June 1944, Van der Haagen writes Martinus Nijhoff a letter in German, with the complaint that the summaries in *Physica* are in English instead of German, ‘in the appropriate manner’. Not the publisher, but the editor (Fokker) gives Van der Haagen the immediate answer, in Dutch, (13 June) that they will be in English, as has always been the case. The publisher (Martinus Nijhoff) repeats that answer (17 June). More than a month later (and after the Normandy-invasion), on 28 July, Van der Haagen answers that all non-German papers should have a German summary, but that English summaries are always allowed. Fokker writes to Verhoef (18 August) that he cannot accept the ‘unclear position’ of Van der Haagen (is he requesting or suggesting something?), and Verhoef answers (21 August) that it is better to go on as before and ignore Van der Haagen. The letter by Fokker to Verhoef on the printing of *Physica* in Zaltbommel is dated ‘April 1945’. Then there finally is a postcard of 26 January 1946 from Fokker to Verhoef with congratulations that the first post-war issue of *Physica* has appeared.

this a generic measure was put into effect at the Department of Sciences. For further publication, *Physica* would require recognition as a scientific journal according to Departmental directives, which were to promote the use of German. In October 1943 Fokker wrote a letter of protest to the offending person at the department, J.K. van der Haagen, against the ‘illegal interference in the language’. As was to be expected, this interference worked counter-productively; in 1943, the percentage of German articles was 11%, still the same as in 1939, but in the last two years of the war, when only the one meagre volume could be published, it went down to 5%. Up until the autumn of 1944 Fokker, Verhoef and Van der Haagen corresponded together about German versions of the non-German summaries of articles, but still the bulk was in English. Van der Haagen’s letters took on a plaintive tone. Fokker thought they could not avoid the directives. Verhoef thought that they could, as nothing was definite and there were no sanctions. ‘It’ll sort itself out if we don’t do it,’³⁸ he wrote. He was familiar with the Resistance.

Wouter Nijhoff Pzn had kept out of all this. During the war he had greater worries to cope with than the *Physica* journal. The antiquarian book trade, which was the most important source of income for the firm, came under pressure due to the cessation of Atlantic trade. It is impossible, here, to do justice to the resistance against the German occupier in keeping open channels of communication—channels controlled by the Germans—to enable purchasing and supply. In time it became absorbed into a wider resistance movement. This we know from an anarchistic member of the family, Karel van Boeschoten (married to a sister of the author), who worked at the time in the Nijhoff antiquarian book trade, and plunged so deep in illegal activity (or should we say: soared so high?) that after the war he was promptly elected to the emergency council of The Hague. Ever since, though, he has been silent as the grave about what these activities were. People learnt to keep silent. We know hardly a thing about the activities, for instance, of Chris Dutilh and Max Westerbeek, except that they were shot for them.³⁹ As far as the director was concerned, what we know came to light during his trial for treason.

³⁸ See note 37 of this chapter.

³⁹ Visser p. 14.

In March 1944, Wouter Pzn was requested to present himself at the Department of Justice, but when he rang the bell the *Sicherheitsdienst* were waiting behind the door, ready to arrest him and take him to Scheveningen prison.⁴⁰ The methods used to interrogate him, and what he revealed, are unknown. It was not until June that they actually formulated the charge, in a document of the *Deutscher Generalstaatsanwalt in den besetzen niederländischen Gebieten*. Together with 22 others, he was accused of printing and circulating *Het Parool*, a newspaper considered by the Germans to be a *Hetzschrift* (a subversive piece of writing), and which enjoyed a circulation of 40,000 copies. Sentences were pronounced in July. They had evidence against Wouter that he had twice circulated 25 copies of *Het Parool* and contributed a thousand guilders to the costs of the newspaper, and they demanded a minimum of eighteen months and a fine of ten thousand guilders (?). They demanded the death penalty for four of the 23. In August the German judge, Dr Oegg, passed sentence in a public court in Utrecht, in the presence of the families of the accused. The *Generalstaatsanwalt* must have wanted to play a nasty trick on the *Sicherheitsdienst*, because his sentences were mild. Nobody was given the death penalty and Wouter only got six months, with deduction of time already spent in custody. This meant that he would be released in September. By then he was in the Cleve prison. On the way to The Hague, he got caught up in the chaos of ‘Crazy Tuesday’, in the advance of the British-American liberation army through Belgium and the southern Dutch province of Brabant. The printing works at Zaltbommel were evacuated, because of the formation of the allied front along the river nearby, for almost six months.

By some coincidence, during these same difficult times Martinus Nijhoff managed to publish volume 21 of the *Œuvres Complètes de Christiaan Huygens*, as well as *Cosmotheoros*⁴¹—and on handmade paper with a watermark. The coincidence was apt. *Per aspera ad astra* is the classical expression: through affliction and ordeal must we pass to reach the stars. Well, didn’t *Cosmotheoros* take us to the stars? With the aid of lenses and pinholes, Huygens compared their light to that of the Sun, and was the first to obtain a fairly accurate impression of their immense distance. In the meantime Fokker was wondering whether the

⁴⁰ Interview of Paul Nijhoff Asser on 1 March 2006, who possessed pertinent family documents.

⁴¹ Andriessse pp. 389–398.

type for *Physica*, stored in the Nijhoff printing works of Van de Garde in Zaltbommel, would survive the bombs. ‘Shall we’, he wondered in April 1945, ‘perhaps be able to carry on from the volume where we left off, with exactly one year’s interruption?’⁴²

He did not have to wait that long. In January 1946, he was already able to start with the publication of a following volume, volume 12, which would consist of 778 pages and 78 articles. Only two articles were in German (these were submitted by a German researcher), and six were in French. From then onwards, the *lingua franca* was clear. It would be English. However, *Physica* did not immediately recover the international standing that it might have enjoyed in this language in the 1930s. Physics research in the Netherlands had virtually come to a standstill (‘it had become a backwater’)⁴³ and there was little of real interest worth mentioning. From Verhoef’s letters to Fokker, we learn that the journal suffered a loss (in 1947 the deficit had reached 4451 guilders) and the number of foreign subscriptions had shrunk greatly. If there were no more subscribers to be gained, Nijhoff would have to stop the publication... It can only have been Wouter Pzn himself who guaranteed it for a couple more years, giving Fokker a decent chance to see whether the journal could be saved. Neither of them then could have foreseen *Physica*’s growth in the 1950s. If he had, Wouter Pzn would never have handed over the ownership, in 1951, to the *Nederlandsche Natuurkundige Vereniging* (Dutch Physical Society), which created a foundation especially for this purpose. This foundation took over the management, with financial support from Philips, had the journal published by North-Holland, and distributed by Nijhoff—a construction that would continue until 1967.

What more is there to tell about post-war physics publications by Nijhoff? There were not very many. Apart from the usual laboratory reports, and the dozen inaugural lectures that were part and parcel of the publications of this renowned house, we find only a symposium

⁴² See note 37 of this chapter.

⁴³ Quote from a conversation on 5 September 2003 with J.M.H. (Anneke) Levelt Sengers—member of the (American) National Academy of Sciences—who soon after the war, and after a brilliant dissertation on critical phenomena in fluids, left the Netherlands and found work at the National Bureau of Standards in Washington, of which she became head of the equation-of-state section. She hasn’t been the only scientist that left. Nicolaas Bloembergen and Abraham Pais and Niko Tinbergen and many others did the same.

report, a very modest journal called *Plant and Soil* that Eppe Mulder (an agricultural scientist from Groningen) began in 1947, and a few volumes of the *Journal de 1604 à 1634*, written by Isaac Beeckman and edited by Cornelis de Waard, which was established in 1939. Were they, in fact, still interested in specialised acquisition? Wouter Pzn provides the answer—though indirectly—when he looked back and wrote, probably 76 years old: ‘Financially β + medicine was actually always better than α . But we didn’t want it. β simply did not appeal to us. Business-wise I am not proud of this. So what!’⁴⁴ To this reluctance we may add laziness. In fact, thanks to nepotism, management had been handed over in the 1950s to Pieter Dijkema. We know from Anton Gerits, who was working at the time in the antiquarian book trade, that this man, ‘not what you’d call an energetic worker’, usually sat reading at an empty desk (‘supposedly keeping abreast of professional literature, but almost always this was light reading’), and that he never refused or published a single manuscript without first asking permission from Wouter Pzn.⁴⁵ In any case, Dijkema could not have been involved in the important decision to publish the *Husserliana*, because this was taken long before the first volume came out in 1950, when he was with the Haarlem company Heirs F. Bohn. These *Husserliana* were brought in shortly after the war by a Franciscan father from Leuven, who knew of only one publisher who should be allowed to publish them: Nijhoff. Hardly light reading, they include all the posthumous writings of the philosopher Edmund Husserl (1859–1938), and the publication is still not complete, even today. (Naturally this showpiece of Nijhoff must be mentioned, although it does not belong amongst the natural science publications. Or does it? ‘The existence of the world is so self-evident,’ we read, ‘that nobody thinks to determine it specifically.’)⁴⁶ Anton Gerits goes on to tell us about the “smokehouse” at the Lange Voorhout in The Hague where, in room 10 of these monumental premises, the director and the head of the antiquarian book trade, Hendrik Kern, sat at desks placed opposite one another, surrounded by books lining all the walls and partly enclosed behind glass doors. ‘There was always a thick cloud

⁴⁴ Letter from Wouter Nijhoff Pzn to Anton Gerits (without date, but probably from 1971); here β stands for mathematics and natural sciences, and α for literature and humanities.

⁴⁵ Gerits p. 74.

⁴⁶ Edmund Husserl, *Pariser Vorträge* [cited on p. 336 in *De Verbeelding van het Denken*, J. Bor & E. Petersma (Eds.), Contact, Amsterdam (2000)].

of cigar and cigarette smoke, for both gentlemen were heavy smokers.' Once when Gerits opened a window, Kern ordered him to shut it again immediately: 'Mr Nijhoff and I do not like fresh air!'⁴⁷

Symbolic? It seems so, for indeed Nijhoff would shortly afterwards disappear into the mists, in spite of the fact that in America trade had revived, and in 1953, the year of its centenary, the firm employed 125 staff. It was the classical story of a family business, in which its members, by thinking only of themselves, lose their vision of the future of the company. Nowhere is the story written down, so we had to seek out a family member to find out. Here are the words of Paul Nijhoff Asser:⁴⁸

This family has quite a history. Let me begin with myself. I was born in 1928 and named after the Amsterdam book dealer Paul Nijhoff, my maternal grandfather. Wouter Pzn was the brother of 'Sis', actually Mia, my mother. At birth I was called Paul Asser, but because grandfather Paul Nijhoff saw the family name in the book business dying out, he arranged, in 1948, to have my name changed to Paul Nijhoff Asser. My uncle Wouter was a handsome fellow, and a great lady's man. He married three times, six years to a local girl, a bit more than one year to someone from whom he quickly became estranged, and from 1932 until his death in 1977 with Sylvia, from America. He had no children by any of these three women, so no son to succeed him as director of Nijhoff. Sometime around 1950 he asked me to come to The Hague, but I felt that it was not for me, that enormous firm with its antiquarian book trade and export business, and I had to disappoint him. To be honest I was not up to the job. I already had my hands full with Scheltema & Holkema, the former firm of grandfather Paul. At the time, just as at Nijhoff, books were not only marketed, but published as well. For example, we published books for the Dutch universities, for which there was a high demand in the 1950s and 1960s, and which also had to be updated and reprinted. These included the *Leerboek der Natuurkunde*, which was edited by Ralph Kronig, and the *Handboek der Wiskunde*, edited by Lauwerens Kuipers and Reinier Timman. But Wouter saw, even then, that finding a successor would be a problem. There was little time to be had. By 1960, someone born in 1895 has a right to a pension.

The young Wouter had, I think, little or nothing to say about the succession. When the elder Wouter died in 1947 his Nijhoff shares went to his heirs. This was 95%, because I think that young Wouter got no more than 5% when he was appointed co-director in 1928. Old Wouter's heirs were his children. In the family they were called 'Pien' (a daughter), 'Pon'

⁴⁷ Gerits p. 68.

⁴⁸ Interview of Paul Nijhoff Asser on 1 March 2006.

(that was the poet), 'Hes' (another daughter), and 'Pim' (the odd one out who, after studying physics, left for the Dutch East Indies). They were about the same age as my uncle, the young Wouter. So to continue the family tradition in Nijhoff, young Wouter's successor had to be found in the next generation. First they thought of the descendants of Pien, who was actually called Emma and was married to Frans Wagner, a first cousin (his mother was a sister of the old Wouter). This Frans Wagner, after a planter's career in the Dutch East Indies, became director of Van de Garde in Zaltbommel. The Wagners always liked to be obstructive during shareholder meetings, and could be a real headache to Wouter Pzn. To begin with they waited until 1951 before even formally appointing him director and, at the same time, making Charles Gilhuys and Pieter Dijkema co-directors for finances and publishing, respectively. Gilhuys, an outstanding accountant as well as an actuary, had already worked in the firm for a long time, but Dijkema had not—they had taken him from another family business in Haarlem. It was only with the greatest difficulty that my uncle was able to win over these Wagners to his cautious policies. But they forced him to take into the business the young Wouter Wagner, born in 1928, and then the young Wim Monhemius, born in 1929 and married to Johanna Wagner. On probation. Only when it was proved beyond a shadow of doubt that neither of them would be capable of leading the book trade and the publishing department, let alone the antiquarian book trade, did the Wagners back down.

After I had declined to come to The Hague and enter the firm, nothing happened for a couple of years. But around the time that my uncle Wouter was planning to retire, in 1960, the family thought of the children of 'Hes', who we have already mentioned. It so happened that 'Hes' had a daughter who had a husband who had a friend who would fit the job perfectly. This was Hartger Hartgerink. He was born in 1908, so already fairly old, even in 1942 when he did his PhD on indigenous education in the Dutch East Indies. That man! Inanity incarnate. I got to know him when they appointed him director of Nijhoff in 1964, alongside Dijkema, and he summoned me as corresponding director of Scheltema & Holkema to The Hague. On that occasion I was the recipient of a reprimand concerning a minor wage increase of a cleaner, which he had discovered in the annual accounts... It was amazing how that man managed, during the 1960s, to drive away all the good people from Nijhoff...

In 1970 Anton Gerits, the antiquarian bookseller, and Paul Nijhoff Asser both resigned. So Paul, who we have just quoted, was director of Scheltema and Holkema, a half subsidiary of Nijhoff. They were by no means the first to tire of Hartgerink and leave.⁴⁹ The first was Hans

⁴⁹ Letter from Paul Nijhoff Asser to the author of 17 June 2006.

Stenfert Kroese, director of Heirs F. Bohn in Haarlem, a full subsidiary of Nijhoff, who published the *Nederlands Tijdschrift voor Geneeskunde*. Key figures such as Charles Gilhuys Jr. and Hugo Brandt Corstius left shortly after. The former had succeeded his father, and the latter had taken over Wouter Pzn's job of travelling for several months each year, to maintain and expand the export book trade. Others followed. The family's decision to sell Nijhoff added further to the general exodus. In chapter 8 we shall say something more about the assets.

CHAPTER THREE

GERMAN SCENES

What happened to the east of the Dutch border is crucial for the remainder of this book. In the last chapter we saw some of the consequences of the German occupation of the Netherlands. Now that we have come to the development of the great Dutch scientific publishing houses, we need to look at the causes of this occupation—causes that must be sought within Germany itself. What enabled the emergence of Elsevier and North-Holland was that publishing houses such as Akademische Verlagsgesellschaft and Springer-Verlag were destroyed by the Germans themselves, as a result of racial policies and the war in which the Nazis had swept them away. We have to address relationships, and interventions.

Let us go back in history—first of all, to the ruins of Berlin. The war has ended two years ago, but much of the city is still uncleared. The badly damaged premises of Springer-Verlag near Potsdamer Platz have been provided with a temporary new roof, windows and doors, and part of the book stock has actually been salvaged from the rubble.¹ Though the building lies in the Russian sector, the opposite side of the street, the Linkstraße, is British. Further along in the British sector a member of the inspection committee of the occupying powers has stumbled across a batch of Springer's scientific journals. This young man has a brainwave: he goes to the Linkstraße and asks to speak to the director. Tönjes Lange and Ferdinand Springer—for there are two directors—are interested in what he suggests, and impressed by the Military Cross he is wearing. So, in exchange for paper, which he can get hold of, and a permit that he can arrange for them, they give him the rights to export their publications. In order to do so, the young man sets up the European Periodicals Publicity and Advertising Company, which will collaborate with a London publishing house under the name of Lange, Maxwell & Springer. On the basis of these exports, Robert

¹ Götze pp. 1, 14.

Maxwell—for it was none other than he—will build up his own science publishing house, Pergamon Press in Oxford, which will flourish and grow and one day will be taken over by Elsevier.²

But let us go back even further—to January 1945, when Lieutenant Maxwell has a platoon under his command at a village 6 kilometres from Roermond, a town in the south of the Netherlands, near the German border.³ This is where the English are expected to cross the river Roer. To beat them to it, a German battalion breaks through the lines on the left bank. Their heavy artillery fire hits the mark, and the fleeing British take refuge in the houses and sheds of the villagers. Maxwell is ordered to retreat, but argues in favour of a counter attack that the enemy won't be expecting. His plan is sanctioned and he goes ahead with a surprise attack 'with no regard for his person' and rescues his comrades in the village. Under heavy fire, the beleaguered Germans retreat to the river. 'Through his splendid example and brilliant offensive the situation has been saved.' Field-Marshal Montgomery is informed of Lieutenant Maxwell's heroism, and pins the Military Cross to Maxwell's chest. Robert (Bob) Maxwell is only 21. Four years earlier he had fled to Britain from Carpathian Ruthenia in the Maramureş. He has by now abandoned his Jewish name, Abraham Leib Hoch.

Two Scenes from 1942

In September the Reichskammer (Imperial Chamber) for literature orders Ferdinand Springer, then aged 61, to leave his firm—he is 'half Jewish', that is he has two Jewish grandparents.⁴ But with customary deference to him, the Reichskammer gives him six months to go. A few months later when he hears from his partner, Tönjes Lange, that there are plans to split up Springer-Verlag and drop the name Springer, Ferdinand informs the Reichskammer that he will leave straightaway. Tönjes Lange and his brother Otto have registered themselves as owners of the firm, but regard themselves as authorised representatives who

² Robert W. Cahn, 'The origins of Pergamon Press: Rosbaud and Maxwell', *European Review* 2 (1994) 37–42 [Quarterly of the Academia Europaea, published by John Wiley].

³ Elisabeth Maxwell (*A Mind of my Own*) pp. 178–179, 67–71.

⁴ Sarkowski pp. 373, 383.

will withdraw, as soon as circumstances permit. Ferdinand retreats to his Berlin villa, but after this is bombed and becomes unfit for habitation, he moves to a country house belonging to friends in Pomerania. It is here that he will be arrested by the Russian Secret Service. A major will ask him his profession: 'I publish scientific literature.'—'Really?'—'Mainly journals.'—'Write down a few titles.' And while he does so, the Russian exclaims—'Stop! I myself have published articles in this journal, and in that one.' An apocryphal story? It's told by a Russian professor who ended up in the Russian Secret Service. It's the story of the sealing of a strategic friendship.

In December, Paul Rosbaud (then aged 46) travels to the Netherlands.⁵ He has told Springer-Verlag that he wants to talk to authors about publications, but not that he intends to contact the Resistance. He wants to make sure the British Secret Service knows that Walter Hicher in Munich is working on iron-pentacarbonyl. He is not sure whether the message he sent to London under his codename Theodor ever reached its destination, but it is vital for people there to know that iron-pentacarbonyl is a poison gas that can penetrate gas masks. His plan is to contact an organic chemist, Jan Hendrik de Boer, who used to work at Philips, but who escaped to England at the beginning of the war and is probably working on poison gas. To find out how to contact De Boer directly, Rosbaud visits his former associates in Leiden, Hendrik Casimir and Anton van Arkel. The latter puts him in touch with the botanist, Marius Hille Ris Lambers, who knows of a safe way to pass on information to De Boer. Once again Rosbaud drops in on Ralph Kronig in Delft, to continue their discussion of publications in nuclear physics, and perhaps to find out about the plans for all the uranium that is stored in a cellar of the Delft Polytechnic. Kronig can't understand why Rosbaud keeps visiting him, but Hajo Bruining, the Philips employee who he subsequently visits in Eindhoven, has realised that, under the pretext of working for Springer-Verlag, he is involved in spying ('a highly dangerous game'). There is no need for him to call on Anton Michels at the Van der Waals Laboratory in Amsterdam, for

⁵ Kramish pp. 142–144; 'Professor A.M.J. Michels, the successor of J. de Boer at the Van der Waals Laboratory of the University of Amsterdam, was one of the very few and one of the most important British agents in Holland.' On p. 261 Kramish acknowledges the help he got from Daan Frank 'in sorting out the elements of the wartime Dutch intelligence networks.'

he is a fellow spy, one of the few to be working for the British Secret Service, and one of the most important.

Paul Rosbaud has learnt to keep quiet.⁶ Having grown up in the backstreets of Graz, as the illegitimate child of a spinster who even on her deathbed wouldn't tell him who his father was, he is forced to lie about his origins, and invents non-Jewish grandparents entered into baptismal registers. He becomes an engineer in Berlin, then works as salesman and editor for the journal *Metallwirtschaft*. After discovering that the owner is a financial backer of a storm troop, he leaves to earn his living as an independent advisor for what in his eyes are respectable journals and institutions. Although he has not begun a scientific career, he is still aware of what goes on in the world of materials research, and knows himself to be 'a hook in a pond of carp'. Ten years before going to the Netherlands as a spy, he already saw that Germany was going the wrong way. Compelled by his sense of honour, compromised already by the lies about his family, he sets out to thwart the Nazis in whatever way he can. When he eventually settles in England after the war, he will work for Maxwell, supplying an invaluable publisher's list to Pergamon Press in Oxford.

A Letter and Three Scenes from 1938

In April, Secretary Thulke of the Reichskammer for literature in Leipzig writes a letter to the Central office of the Reichskammer in Berlin:⁷

Dear Karl-Heinrich Bischoff—I hereby return the emigration request of Walter Jolowicz. I have discussed it with the head of the antiquarian book trade section and wish to inform you of the following. What Jolowicz writes about the state of the German book trade is largely correct. [But] the bibliophile antiquarian book trade of [Gustav] Fock, whose sales have fallen again, is so much worse off than the general and scientific book trade, that we may assume that in 1933 and 1934 his firm sent a large part of the stock abroad (to North America and Japan). Until now we have no proof of this, but anyone familiar with the antiquarian book trade knows that both these gentlemen, Jolowicz and Jacoby, are extremely sophisticated businessmen. So if Mr Jolowicz says that he [abroad] will promote the interests of German exports of antiquarian

⁶ Kramish pp. 3–27 (Chapters 1–5), which are concluded by: 'In Germany [...] there was the opportunity to inflict a few wounds on the Nazi state and on Adolf Hitler. And the desire to wound had become an obsession.'

⁷ Lorz pp. 108–110 [our translation of the German].

books, naturally this cannot be taken seriously, because he will just be working for Fock. Such veiled tactics are truly Jewish. He will be working for Fock and using this method to make the position of the firm in Germany even stronger. [...] Mr Jolowicz knows exactly how to do this. I have not yet discovered what separate property his wife owns, but you can see yourself that Walter Jolowicz has only 36,000 marks invested in Fock. Recently I have gone into the matter [of the capital] and have been comparing figures. It must be realised that the Akademische Verlagsgesellschaft is entirely in the hands of the Jews Jolowicz and Jacoby. According to the Register of Companies, these Jews deposited a capital of 20,000 marks, but by manipulation managed to put another 420,000 marks into a separate account, thereby saving on tax. This was permitted at the time, but you can see how cunning these people are. Mr Jolowicz once let slip that he would have to pay an enormous amount in taxes if he left. That is probably the main reason why the Jews have decided not to leave yet, and now they are trying to remove their capital bit by bit. [...] If you look at the shareholders list of Fock booksellers, then it strikes you immediately that Paul Dünnhaupt, a printer in Köthen, is listed there with the extraordinarily large sum of 229,000 marks. Again, it cannot be proved, but the sparrows shout it off the rooftops of Leipzig that Dünnhaupt has invested this sum only nominally in the books, and pays tax over it, so he can do printing assignments for Fock and the Akademische Verlagsgesellschaft. Only a short while ago, the printing office and publishing office exchanged a nominal sum of 40,000 marks in shares. We can assume that this all has to do with taxes that Dünnhaupt has paid in the meantime. The joint owners Jolowicz and Jacoby are not mentioned any more [in these financial documents], only the Aryans who act as managers: Willy Erler and Johannes Geest. As far as I can ascertain, they have only acted in this capacity since 1934. Undoubtedly they are professional but, of course, they have only been installed to serve the interest of the Jews. So, knowing all this, it is not surprising that they have put in a request to be allowed to take with them 100,000 marks worth of antiquarian literature abroad. Under cover of bringing in foreign currency people will try just about anything. Even so, I wouldn't stop Mr Jolowicz making such a move. If I were you, I'd use the opportunity to get rid of all three Jews at once, father and son [Leo and Walter] and [Kurt] Jacoby. People would agree to 200 to 300,000 marks, just like that. We'd be doing a great service to the German book trade if we got rid of the last full Jews! We agree on the goal: to get rid of the Jews. And with a bit of luck it will be Jolowicz himself who places the entire firm into pure Aryan hands.

Three months after this letter, one evening in July, Paul Rosbaud (then aged 42) knocks on the door of Lise Meitner's apartment.⁸

⁸ Sime pp. 192–207 [especially p. 196 on Debye's role in the escape of Lise Meitner: 'He was politically adept, non-German and head of a major institute, altogether less

Meitner (then aged 59) lives in a villa of the Kaiser Wilhelm Institute in a south-western suburb of Berlin. She has stayed on at the physics laboratory until 8 o'clock, to correct an article describing the research she is leading on neutrons from a radium-beryllium source. Now it is 10.30, the time that she has arranged with Rosbaud to come and fetch her. ('I gratefully remember the way you went through my rooms in Dahlem one last time, you were so friendly and full of understanding, and the way you stuffed all sorts of bits and pieces into my suitcases.') Otto Hahn (then aged 58) has already been there for one and a half hours to help her pack, and he gives her his mother's diamond ring 'in case of need.'

In Berlin, apart from Otto and Paul, only Max von Laue and Peter Debye, the Director of the Kaiser Wilhelm Institute, know that Meitner is leaving. Since the annexation (*Anschluss*) of Austria in March, she is subject to German law, which stipulates that Jews are to be dismissed. And because scientists have been forbidden to leave the country, she must go unnoticed. She can only take two suitcases of clothes—a crate of books will be sent on later. Rosbaud drives the two of them in his Opel to Hahn's house, where she spends the night, and early next morning he drives her to the station. She feels nervous now that she must leave the country after 31 years, and hardly dares to enter the train. Inwardly she must feel more torn than we can imagine, but she hides it and just makes small talk.

Dirk Coster (then aged 49) is waiting for her in a first-class carriage, where they act as though they are strangers and just nod at one another. But she knows this physicist from Groningen well, even before he discovered hafnium. Meanwhile Coster has spent the night with Debye, having arrived the evening before with the vital entry visa for Meitner, which he and Adriaan Fokker had managed to obtain from the Ministry of Justice in The Hague. Coster and a neighbour, Ebel Ebels, who has estates at Nieuwe Schans, have already arranged Meitner's arrival beforehand with the frontier police—for they intend her to slip out of Germany this far north via a little-used railway line. When the train leaves Berlin, they are unaware that Kurt Hess, Meitner's neighbour in the Institute's villa in Dahlem, has informed the security service that his neighbour is about to leave. But the authorities there

likely than others to arouse suspicion with his extensive international correspondence.']; Kramish pp. 48–49.

only take action after Meitner and Coster are safely in Groningen at 6 o'clock that evening.

And now we come to a sequel to Thulke's letter:⁹ In December Walter Jolowicz is released from Buchenwald, a concentration camp not far (80 kilometres) from Leipzig. It lies on the hill above Weimar where—as the name indicates—a beech wood ends. Jolowicz, prisoner 4777, suffers from exhaustion or from the frequent beatings he got during his work with tree-stumps up there in the snow-covered wood. Who can say what treatment has been meted out in those six weeks? He is emaciated and covered in dirt, trembles, and is beset by fainting fits—all this, at any rate, is plain to see. Insomnia has plagued him since his return to Leipzig, and he is beset by the financial and legal problems surrounding his emigration. He wants to leave, but is not permitted to do so and must report to the police each month, although he has deleted his name from the Companies Register, and relinquished his share in the Akademische Verlagsgesellschaft, just as his father Leo (then aged 70) and his brother-in-law Kurt Jacoby (then aged 45) have done. He avoids the antiquarian book shop on the Sternwartenstraße like the plague, leaving his house in the Ferdinand Rhodestraße at the crack of dawn to wander the town all day, as he is afraid of being picked up again.

Surely it can only be malice that moves the New Order in Germany to take such measures against a small group of men—men who founded the Akademische Verlagsgesellschaft and made it into what it is—*after* their firm has been stolen? It will take two more years, by which time Leo has died of misery and been buried in Leipzig, before Walter and Kurt will be allowed to travel to Russia, just before war breaks out on the eastern front, and via Japan they reach America, where they will found Academic Press. The Akademische Verlagsgesellschaft, set up in 1906 for the publication of academic books and journals in the field of natural science, had become the second largest in the country.¹⁰ In 1931, their jubilee year, they had sales worth two million marks, 70% from abroad, and published 26 journals, while Springer-Verlag—the

⁹ Lorz p. 113; we render a passage in the original: *Den Erlebnissen in Buchenwald folgte ein schwerer Nervenschock, er erlitt Ohnmachtszustände und Angstanfälle. Oft verließ er in den frühesten Morgenstunden die Wohnung, um stundenlang spazieren zu gehen und sich dann irgendwo versteckt aufzuhalten, aus Furcht, er könne wieder geholt werden. All das machte ihn schlaf- und ruhelos.*

¹⁰ Lorz pp. 97–102.

largest—had sales worth ten million, 65% from abroad, with 125 journals. (Oh, that jubilee celebration in the Free Masons Lodge ‘Minerva’, with music by Mozart and a speech by Ostwald, their famous author, family friend and Nobel Prize winner!) But in 1938, that disastrous year, their sales would be reduced by half, while those of their great competitor in Berlin would remain the same.

The authorities come down even harder on Kurt Jacoby. Married to Walter’s sister Agnes Jolowicz, Leo’s son-in-law has built up an outstanding publisher’s list, with works by Svante Arrhenius, Felix Bloch, Louis de Broglie, Pierre Curie, Peter Debye, Adriaan Fokker, Walter Heitler, Jacobus van’t Hoff, Walter Hückel, Georg Joos, Hendrik Kramers, Max von Laue, Wilhelm Ostwald...¹¹ He owes this success to a combination of astute business talent together with an intuitive understanding of the new chemistry. Like his father-in-law, he comes from the east, from Insterburg in Eastern Prussia—old Leo comes from Posen (at that time, and also now) in Poland and known as Poznan. It is Kurt who is the kingpin of the Akademische Verlagsgesellschaft, and not Walter, the still inexperienced young brother-in-law, and this will play a role in his treatment. Like the brother-in-law, he is detained for a while in a concentration camp after the *Kristallnacht* pogrom of November, and is later released. But just before Christmas he is arrested again and, ‘for his protection’, put into the First Penal Prison for interrogation, where he is accused of sabotaging the *Arisierung* of the publishing house.¹² When this becomes known in the publishing world, Daan Frank—a remarkable man who we shall meet in the chapters to come—immediately boards the night train from Amsterdam to Leipzig and tries to bail Kurt Jacoby out of prison.

There is another scene from 1938 which, though short, spells ruin.¹³ In the same December Otto Hahn discovers that irradiated uranium contains *barium*, which is strange. After Meitner’s departure he had continued their joint research, irradiating materials with neutrons and observing, with the aid of chemical analysis, how they have changed.

¹¹ Geest & Portig pp. 11–230 (Catalogue of the Akademische Verlagsgesellschaft).

¹² Lorz pp. 113–114; we render a passage in the original: *Ebenso nach Buchenwald gebracht, und wieder entlassen, wurde er aber am 22. Dezember 1938 erneut in Schutzhaft genommen. Es hieß er sabotiere die Arisierung der bisher von ihm (mit)geleiteten Akademische Verlagsgesellschaft, so daß bei Freilassung eine Fortsetzung seines ‘staatsabträglichen Tuns’ zu befürchten sei.*

¹³ Libby pp. 46–51; Kramish p. 50.

His assistant, Fritz Straßmann, confirms that this results not in any of the actinide series, but in *barium*, an element that, in spite of the name (βαρύς means heavy), is much lighter than uranium. Therefore atomic nuclei of uranium disintegrate after penetration by neutrons—and do not, as expected, become heavier. There has already been speculation about this break down of an atomic nucleus, but now there is proof ... Hahn and Straßmann immediately write an article on it, which they pass on to Paul Rosbaud, their confidant, and the man who is always around. And Rosbaud just manages to get it into the January issue of *Die Naturwissenschaften* of Springer-Verlag, after persuading the editor, Fritz Süffert, to publish it at the expense of another article. Before the Nazi authorities get wind of this, Hahn and Straßmann's discovery is already known abroad. Meitner, who has travelled on from Groningen to Stockholm, now knows that the uranium nucleus has broken in two—she will use the word fission—and will make this known across the world, as well as her estimation of the gigantic energy that will be released with this fission. In *Nature*.

Two and a Half Scenes from 1933

Once more we go back in history, this time to the year in which the New Order in that country has just begun. Much may be said about it, but the real question is: who belongs, and who doesn't? According to Michael Walzer, a contemporary sociologist born shortly after this ominous year, every political community has to answer this question.¹⁴ 'For,' he tells us, 'a community is ordered—constitutes itself—according to the answer it gives to this question, or rather, according to the procedure it follows to decide which answer counts—even if this decision is not definitive and no absolute dividing line is drawn between who belongs, and who does not.' But we are left with that incomprehensible question, which for centuries has aroused curiosity and sophistic subtlety, but also bitterness, intrigue, dispute, thirst for power, barbaric superstition, fury of persecution, blind and bloody fanaticism: do the Jews belong?

¹⁴ Michael Walzer, 'Exclusion, Injustice, and the Democratic State', *Dissent* 39 (1993) 55–64 [see also Walzer's *Spheres of Justice* mentioned in the Bibliography].

The New Order is accepted without discussion, in a procedure that has the appearance of legality.¹⁵ In April a law is enacted ‘on the dismissal of civil servants with a permanent appointment’. Only in §3: ‘Beamte, die nicht arischer Abstammung sind, sind in den Ruhestand zu versetzen’ does it become clear what is really meant. (Civil servants not of Aryan descent must be sent into retirement.) And the explanation quickly follows: ‘Als nicht Arich gilt...’ not Aryan is anybody who does not descend from Aryan parents or grandparents, which in practice means: Jewish. It is sufficient that one of the parents, or one of the grandparents is non-Aryan. Even if the designation ‘Jewish’ is avoided, ‘non-Aryan’ promptly takes on an ominous overtone, and people prefer ‘n.a.’

Albert Einstein (n.a.) has seen it coming.¹⁶ The world famous physicist has been to America to discuss an appointment in Princeton and on his return has taken the boat, not to Bremerhaven but to Antwerp, to avoid going to Germany. Even before the law becomes known, from the Belgian coast he writes a letter of resignation to the Academy of Sciences in Berlin. By then storm troops of the New Order have already plundered his country house near Berlin, in search of suspected communist weapons and munition. Fritz Haber (n.a.), too, moves fast, and writes in April to the Minister of Education that he will select his staff, not on the basis of their race, but on their professional and personal characteristics. ‘The pride with which I have served the German Fatherland my whole life long compels me to hand in my resignation.’¹⁷ He is 64, Director of the Kaiser Wilhelm Institute for physical- and electro-chemistry, renowned for his ammonia synthesis from air, and infamous for his development of poison gas and of nitrates that are essential for explosives. However, countless other n.a. public servants have not seen the law coming.

The younger Eugen Wigner (n.a.), also from Berlin, and Felix Bloch (n.a.) from Leipzig, are taken by surprise and see no easy way out, although they came from abroad (Hungary and Switzerland, respectively).¹⁸ But it is in Göttingen, the Mecca of science in Germany,

¹⁵ Sarkowski p. 324; Beyerchen 11–14.

¹⁶ Brian pp. 241–255 (Chapter 25); Pais pp. 312–318, 449–452.

¹⁷ Beyerchen p. 42.

¹⁸ Beyerchen pp. 15–39 (Chapter 2). Erwin Schrödinger, mentioned in the list on p. 48, wasn’t a Jew; he hated the Nazi’s so much, though, that he didn’t return to his Berlin position when he had left for holidays in Tyrol in July 1933; he wrote a letter

that the heaviest blow falls. James Franck (n.a.) is so shocked when he reads of his dismissal that he declares in a local newspaper that ‘we Germans of Jewish origin are treated as foreigners and enemies of the Fatherland’, but is advised by friends to keep quiet. The sickly Max Born (n.a.) responds to the news by quickly arranging an appointment in Cambridge, and fortunately he is famous enough to be able to do so. Richard Courant (n.a.), still an outstanding professor in Göttingen whose *Methoden der mathematischen Physik* has been published by Springer-Verlag, tries to fight the dismissal, only to realise the following autumn ‘that there is nothing for it but to emigrate, although my youngest son appears unable to understand why he is not allowed to join the Hitler Jugend.’ Besides these three great men, in Göttingen brilliant young scientists are also notified of their dismissal: Paul Bernays (n.a.), Felix Bernstein (n.a.), Werner Fenchel (n.a.), Viktor Goldschmidt (n.a.), Walter Heitler (n.a.), Paul Hertz (n.a.), Kurt Hohenemser (n.a.), Heinrich Kuhn (n.a.), Hans Lewy (n.a.), Emmy Noether (n.a.), Lothar Nordheim (n.a.), Wilhelm Prager (n.a.), Eugen Rabinowitsch (n.a.), Edward Teller (n.a.), and others. Almost nothing of these drastic expulsions appears in the controlled press, but lists will be circulated in London with the names of 1800 scientists who have lost their jobs and are now without means of support. In fact, there are more than 2400, with no less than two thirds from the natural and medical sciences and one fifth from the social sciences and law. Complete professional fields lose their elite.

Entr’acte. In Leipzig, city of books, the story is the same. The police make no effort to intervene, and never report what is happening. During May Day celebrations books are thrown out of the People’s House on the Zeitzerstraße and burned in the courtyard—books from a house with a name like this are held to be communist or Jewish, or both, precisely which no-one bothers to find out in the general plundering. Students are involved as Leipzig is also a city of students, and Joseph Goebbels (then aged 35), Minister of Propaganda, knows why: ‘If students in university cities commit the filth of recent years to the flames, then this is a powerful symbol. But gentlemen (sic) students, you only have the right to burn filth if you undertake at the same time to prepare the

of resignation from Tyrol and travelled to Oxford, where Frederick Lindemann had promised him a position (Schrödinger p. 36); since he was expected to get a Nobel Prize for his development of quantum mechanics, ‘Berlin’ refused his resignation.

way for a new German spirit. Leipzig is the centre of German book production, and as such of enormous cultural significance.¹⁹

Hitler has risen out of the nameless masses, a man like anybody else, and yet like none other. For 43.9% of the population this demonic, vulgar fellow comes as a saviour. In his capable hands all will be well. Furthermore, in secular form the old chiliadic dream again arises: the 'Thousand-Year Empire' has arrived he will lead the masses in. What does the constitution matter, or democracy? Racial thinking spreads as an ideal through the community that wants no more have to do with status and class. Men with simple roots, with a deep-rooted hatred of the bourgeoisie, the rich and the profiteers, want what they feel is their due. They stand with their fists and cudgels at the ready; lashing out to the left and right in the police cellars and concentration camps.

The charismatic leader cannot avoid granting an audience to the 75-year-old President of the Kaiser Wilhelm Gesellschaft, Max Planck, but hardly listens: 'Please understand me, Professor, I have nothing at all against the Jews as such. But the Jews are all communists and these are my enemies.'—'But Mr. Chancellor, one must make distinctions.'—'That's not right. A Jew is a Jew. All Jews cling together like burrs. Wherever one Jew is, other Jews of all types immediately gather. The Jews themselves should have produced distinctions. Since they have not, I have to proceed against all of them on an equal basis.' He speaks with ever increasing animation until he is in a rage.²⁰

'I have to proceed against all of them on an equal basis.' Then Hitler moves on to another topic. The President falls silent, then leaves. When he speaks of this audience to others, the story quickly spreads that Hitler is said to have remarked that Germany must just do without science for a bit, if it were true that dismissing Jewish scientists would destroy science in Germany. And later, at the end of his opening speech when Max Planck chairs a meeting of the Kaiser Wilhelm Gesellschaft, he listlessly raises his right arm, only to let it fall again halfway, raises it a second time and lets it fall, then raises it once more, this time to shoulder height and utters the words 'Heil Hitler'.²¹

¹⁹ Lorz pp. 103–104.

²⁰ Beyerchen p. 43.

²¹ Beyerchen p. 1 [according to the testimony of Paul Peter Ewald].

By way of conclusion, we have something very fitting to recall from the Germany of the First World War: Max Planck, discoverer of the quantum of radiation and founder of the quantum theory of matter, is honoured on his sixtieth birthday. He is addressed by Albert Einstein. Both are in tails. Planck has stuck the last remaining strands of hair over his crown; Einstein's sticks out all over the place. 'What is it in science that spurs us on?' Einstein asks. He supplies the answer himself: the urge to escape from the harshness of everyday life, from the emptiness of perpetually changing desires in which we find no comfort:

It moves us, the sensitive ones, towards a world of objective observations and concepts. It is like the irresistible longing that drives the city-dweller from his clamorous, cluttered surrounding up into the quiet landscape of high mountains, where the pure air affords a glimpse of calm contours that seem to have been created for eternity. With this negative motive there belongs also a positive. One way or another, man tries to construct a simplified and orderly image of the world, so that this may to some extent have a place in the world of his experience. By constructing this image, and through the image itself, he comes to the core of his inner being, and to the stillness and certainty that are not to be found in the narrow-minded sphere of turbulent personal existence.²²

Einstein, here, is referring to himself. For him, visual reflection leads as a matter of course to ideas on symmetries. What deviates from this, what is complex and obscure, he prefers to put aside. Can we follow him in these thoughts of calm contours that seem created for eternity? To some extent we can, because we too will find it unnatural that space becomes smaller as a body moves faster in it (which is what the Lorentz-transformation suggests). But then? Will we be able to go a step further and say that the body 'therefore' acts upon that space (on time-space, to be precise) and that it makes no difference whether the mass of that body is inertial or gravitational (that is to say: moves steadily forward or falls)? Someday we shall all know this, because it will be taught in school: time and space and material bodies do not exist separately from one another—without matter, time and space lose their meaning. 'But', as Spinoza knew, 'anything that stands out is as difficult as it is rare.'²³

²² Einstein pp. 169–170 [our translation]; Holton gives a slightly different version (p. 395).

²³ Curley p. 617 [in the original Latin: '*Sed omnia praeclara tam difficilia, quam rara sunt*'—final sentence of Spinoza's *Ethica*].

Let us look now at Einstein's discovery of the general theory of relativity: a theory of gravity—the theory of gravity. He began by constructing a theory that is symmetrical for all inertial systems, therefore with a symmetrical space tensor and a symmetrical motion tensor. Only after several years did he realise that he would not be able to work it out in this manner, so he abandoned the requirement for symmetry. In November 1915—he was now 36—he demanded only that these tensors be co-variant (which means that under every variation of time and space, they do not change form) and that the space tensor adds up to 1. Only then did he get a grip on his problem, and could not stop—sending visitors packing, and eating soup from a filthy pan, with an egg thrown in—until he had solved his equations. He found that the point of proximity of the elliptic orbit of the planet Mercury around the Sun (the perihelium) must rotate at 43 arc seconds per century, and it made him tremble with excitement. For this was the well-known but unexplained deviation from Newtonian theory, according to which a perihelium must stand still. His equations seemed to give a complete and correct explanation for the effect of gravity...²⁴ Could something be deduced that was still unknown? He calculated the angle under which light ('that travels in straight lines') would be bent through the Sun's mass, and found 1.7 arc seconds at the Sun's edge.

Lorentz in Haarlem had followed the mathematics of Einstein in Berlin, step by step. The two were in constant correspondence, so he was able to pass on the prediction of 1.7 arc seconds to Arthur Eddington in Cambridge. This astronomer, unable to receive any messages from Germany during the war, immediately planned to investigate during a solar eclipse to what extent starlight actually bends at the Sun. It would come to be known that it was close to 1.7 arc seconds, for this was the scientific triumph of the century. But do people know that Lorentz was the first to tell this to Einstein? Lorentz the Dutchman as messenger.²⁵

²⁴ Brian p. 91: 'He was euphoric for days afterward, having achieved in the general theory of relativity what some consider as the supreme intellectual achievement of the human species.'

²⁵ Pais pp. 266–274.

CHAPTER FOUR

ELSEVIER'S VENTURE

In 1936 the Nobel Prize for chemistry went to Peter Debye, a Dutch citizen.¹ He was director of the Kaiser Wilhelm Institute in Berlin-Dahlem, a position once held by Albert Einstein, and as such was expected to accept the New Order in Germany after 1933. He was awarded the prize for unravelling molecular structures with the help of scattering experiments. However, this is irrelevant. More to the point is that the Swedish Academy of Sciences, in its recognition of his fundamental contribution to chemistry, drew attention to the fact that he was a foreigner in Nazi Germany, and a couple of years later the problematical nature of his status emerged. In spite of the understanding that he had shown for the regime, he did not wish to take German nationality and had no other choice but to emigrate.

‘Insofar as the mind understands all things as necessary, it has a greater power over the affects, or is less acted on by them.’ [Proposition 6 from Part 5 of Spinoza’s *Ethica*].² Or to put it another way: let us see the dangers that threaten us for what they are, and conceive how they may be avoided or overcome, with presence of mind and strength of character. It is uncertain whether the 32-year-old publisher Johannes Pieter Klautz from Amsterdam knew this classic text. Certainly he had a strong character, and was observant as well. Shortly after 1936, when the international sport world in Berlin was indulging in Olympic cordiality, this sportive-looking blond fellow travelled to a number of publishers in the German-speaking regions.³ His business aim was to publish science books in German—then the science language par excellence—but the matters not related to business that kept cropping up in his discussions moved him deeply, and gave him other ideas. According

¹ Biografisch Woordenboek van Nederland **1** pp. 129–131 [Petrus Josephus Wilhelmus Debye, Lemma by H.A.M. Snelders]; although Debye was born in the Netherlands (in 1884 in Maastricht), he was only one year (1912–1913) professor in this country (in Utrecht); his research was mainly done in Germany (until 1940) and the U.S.A. (until his death in 1966); in Germany and the U.S.A. he spelled his name without ij as Debye.

² Curley p. 599 [Spinoza’s *Ethica* VP6].

³ *Elsevier 100* p. 133.

to a book trader, to whom he spoke in Basel, the wounds that Germany was inflicting upon itself at this time could only be compared to the wounds of the thirty-years-war.⁴

It is no longer a question, he noted down, of whether or not a book should be published. What is happening in Germany concerns the whole world. The regime there is driving its scientists out of the country, obsessed as it is with the idea that every problem must be solved in a new manner, namely with a loud mouth, with lies and half lies, vindictively, and full of contempt for the liberal and humanistic values that we cherish. 'It would be a disaster if such ways were to gain currency with us [in the Netherlands]. One day historians of the twentieth century will ask themselves whether the refugees of this time found material and spiritual shelter with us, or if we disregarded our tradition of hospitality. We may contribute to an important degree to the answer of this question. A publisher is not a grocer. This being so, he must now become aware of his spiritual task!'⁵

Klautz came to realise that there was nothing to be gained for him in this barbaric Germany. He understood that he must be elsewhere, in the world beyond its borders, and must acquire rights to scientific books in German that sold well so as to be able to publish them in English. He must employ in the Netherlands those 'no longer welcome'—being angry at the euphemism—Jewish scholars, who had to seek safety, for the translation and editing of these works. We shall see how he immediately put this idea into practice, but to describe his manner of working, we first quote one of his star authors, a writer of novels:

I have seldom come across a publisher who approached a manuscript with such gut feeling, such sheer intuition. He'd leaf through it, lay it down on the table, look at it, read the heading once again, then when all that was left was for him to smell it, he'd say: tell me about it. Intuitive people have a certain vulnerability, especially if they have to hold their ground in a world of economists. But a publisher is not an economist, at least not first and foremost. He recognises that he is dealing with something spiritual, that books are written not by a committee, but by an individual, a rarity, a creative spirit... It is precisely this recognition that can bring him great financial success.⁶

⁴ *Elsevier* 100 p. 135.

⁵ *Elsevier* 100 pp. 137–138.

⁶ Klautz pp. 276–277.

Johannes Pieter Klautz, who assumed the first name 'Ted' after the war, was born in Deventer in 1904. After completing his secondary education he moved to Amsterdam to study social geography.⁷ However, this study held little fascination for him. No study would have, for there is little in his life to indicate that science held any real interest for him. He tended towards the occult, the non-sensory observable, as we see in *Tussen schemering en dageraad* (*Between Dusk and Dawn*), which he wrote in later life. The university dropout very soon found his way into the world of journalism, and for two years he wrote news articles for the daily newspaper *de Telegraaf*. He fulfilled his military service in a series of summer courses in the mounted artillery, where he developed considerably more respect for the tempestuous steeds than for the light pieces of artillery they had to pull, and greatly enjoyed a gallop and the tattoo across the moor.⁸ In 1927 he answered an advertisement of the publishing company Elsevier, which was looking for an executive secretary with a university education, business flair, literary talent, knowledge of modern languages and of modest character. To be sure, he met neither the first nor the last requirement, but the director engaged him straightaway. His charm, flair and directness had paid off. Or was it his detective's nose? During the interview he had got the director, Herman Robbers, thoroughly hot under the collar when he revealed that he knew that Elsevier had a bank debt of 300,000 guilders—something that was supposed to be an absolute secret!⁹

First we must clear up any misunderstanding that the Elsevier of 1927 was the same house that had existed in the seventeenth century and which had gained fame largely thanks to the cousins Bonaventura and Abraham Elzevier, printers for the Academy in Leiden.¹⁰ The last business of this legendary publishing family was wound up in the beginning of the eighteenth century. But the name remained. As no one could lay claim to it, when Jacob Robbers opened his bookshop and publishing house in 1880, on the Boompjes in Rotterdam, he considered it opportune for developing the business.¹¹ A while later, he thought it

⁷ *Elsevier* 100 pp. 119–122; Klautz p. 9 ('In the elementary school I was taught that there was occasionally a brownie on our shoulder that whispered what we had to do when we didn't know.') and p. 13 ('Why not have fun and try something else? I am fed up with my study.')

⁸ Klautz pp. 20–32.

⁹ Klautz pp. 12–17.

¹⁰ Hartz *passim*.

¹¹ *Elsevier* 100 p. 7.

would be even more opportune to establish himself in Amsterdam, the centre of the Dutch book trade. This grandfather of Herman Robbers found a property on the Nieuwezijds Voorburgwal, at number 64, and moved there in 1887.¹² Apart from *The Principles of Symmetry* by Frans Jaeger, Professor of Chemistry at Groningen, which was published in 1917, he did not publish any scientific works, but kept to high-profile novels and critical prose, starting with the works of Eduard Douwes Dekker (pen-named Multatuli). He was the first in the Netherlands to begin an illustrated monthly magazine, and relied for the rest on the income from a voluminous book of reference. Shortly after 1880 he had managed to obtain the rights to publish the 16-volume encyclopaedia that an erudite church minister, Antony Winkler Prins, had compiled almost on his own.^{13,14} He had also acquired rights for the use of illustrations from the major German book of reference that was published by Brockhaus, the *Conversations-Lexikon*. His *Winkler Prins*, of which four editions had appeared in 1927, turned out to be the financial carthorse of Elsevier and would remain so until well into the twentieth century.¹⁵ But in spite of these prestigious publications, which are not of further interest to us, the publishing house was in very poor shape by the time grandson Herman Robbers took on a new executive secretary.

Ted, as we shall now call him, was put straightaway onto the preparation of the fifth edition of the *Winkler Prins*, with the idea that he would learn through experience the tricks of the publishing trade—the significance of paper, typography, type, print run, investment, price, and how to approach sales. Lack of solvency and rising competition made the new edition a nerve-racking affair, but the man thoroughly enjoyed this as well. ‘When you realise that you are in Leipzig, that you will be 25 the following day and that (together with Herman

¹² *Elsevier 100* p. 8.

¹³ *Elsevier 100* p. 14.

¹⁴ Lunshof *passim*.

¹⁵ *Elsevier 100* pp. 125–127 (‘It was decided that the editor-in-chief, Jan Pieter de Vries from Leiden, would be assisted by two Belgian scholars, August Vermeylen and Jozef Verschuere S.J., in order to get access to the market in Flanders, and to make sure that no articles were published that could offend Roman Catholics.’); about the fifth edition Klautz p. 56 (‘The real question was how to publish a 16-volume encyclopaedia when one is almost bankrupt, thus virtually without capital.’), and about the forward sale of still another, sixth edition, during the war: Klautz pp. 69–71 (‘We had no idea how the new edition would look like, but some 40 thousand Dutchmen blindly trusted Elsevier and paid millions in advance, millions that we put on a deposit for our projects after the war—a lot of this money must have been black.’)

Robbers) you are on your way to an appointment with Hans Brockhaus, the director-owner of a famous publishing house, then your happiness knows no bounds.¹⁶ They were there to negotiate the use of land maps. In the boardroom Ted saw two pictures on the wall, one of a handsome middle-aged woman and one of an ugly little man, and he asked about them.

They hang there by way of a lesson [said Brockhaus]. The one is Johanna Schopenhauer. She wrote novels, not of any great quality, but they were bestsellers and that is why we held her in esteem. One day she showed us a philosophical work by her son, an eccentric character. We saw nothing in it, but what do you do if you wish to please your best selling author? Reluctantly we published it. Today nobody knows Johanna Schopenhauer any more, but the works of her son Arthur are still always in demand. They contribute to the reputation of our house.¹⁷

Is this story in Ted's memoirs supposed to tell us that he immediately recognised the value of science publications? For let us make no mistake: this Arthur Schopenhauer might also have been able to write well, but first and foremost he was a serious man of learning who distilled a hard, somber metaphysics from the best insights of his time in chemistry and physiology, and who had a whole-hearted aversion to modish boasting about ideas as if these were facts. Most probably Ted did not yet recognise this at the time, but rather seven years later, in 1936, when he was in Leipzig again on business. For him it was more the difference between quick and easy profits and the enduring pleasure of stronger sustenance.

Besides, at the time Elsevier could certainly do with some quick and easy profits. So it was lucky that Ted turned out to have an eye for fiction that sold well. For instance, he immediately spotted a bestseller in a biographically-tinted novel, written by the unknown Madelon Szekely-Lulofs, which Herman Robbers thought only so-so and did not really want to publish. Ted thought of a better title for the book, *Rubber*, sold ten thousand of them, and after that almost another hundred thousand more were sold in Germany under the German title *Gummi*, where a play was made of it.¹⁸ Meanwhile, it was abundantly clear to the Robbers family, who owned all the Elsevier shares and had abandoned plans in 1930 to celebrate the fiftieth anniversary of the company due to lack

¹⁶ Klautz p. 55.

¹⁷ Klautz p. 58.

¹⁸ Klautz pp. 40–49.

of funds, that he was just the man to make the company flourish once more. At the end of that year the commissioners appointed Johannes Pieter Klautz as director, starting from January 1931, to succeed the elderly Herman 'a distinguished figure, with a white goatee and lorgnette,' together with the young scion John Robbers, 'a dry fellow with a grating voice', to keep an eye on the till.¹⁹ For this Ted received a salary of 3000 guilders, and John 4200 guilders.

With a bit of good will, we can see in *Natuur en Techniek* his first attempt to build up a science list. Not that he had much success with it. It was an illustrated popular-science monthly that came onto the market in April 1931, and despite the good reviews, it attracted few readers. The losses were high: in 1932, for example, as much as 24% of the total turnover of 19,500 guilders. In comparison: the turnover of the *Winkler Prins* was 35,600 guilders, and of *Rubber* 32,400 guilders.²⁰ So it was not financial, but opportunistic or else strategic considerations that made him think seriously a couple of years later about setting up a science list.

It began in 1931, when he heard from an Amsterdam bookseller that there was still demand for a book on childhood illnesses written by a man called Finkelstein, that this book had sold out but Springer-Verlag would not be re-printing it. Could Elsevier take over publication? It was suggested that he have a word with Maurits Dekker.²¹ This thirty-seven-year-old chemist and microbiologist, who had started up the successful bookshop Dekker and Nordemann on the Oudezijds Voorburgwal in Amsterdam, also imported books from Germany. He would be able to give him valuable information on science publishing in that country. We know that Ted walked straight over to Maurits, but not what was said exactly.

Does it come as a surprise that twin giants are able to create a myth? In any case this is exactly what happened when Elsevier and Interscience both went on to become great science publishing houses, which were to compete with one another. In his memoirs Ted Klautz tells us that 'in fact, they were just waiting for one another' and, if he had so wished, he would have been invited by Kurt Jacoby of the Akademische Verlagsgesellschaft (to do business).²² Towards the end of his

¹⁹ Klautz p. 14.

²⁰ *Elsevier 100* p. 128.

²¹ *Elsevier 100* p. 141.

²² Klautz pp. 80–81.

life, in his turn, Maurits Dekker writes that he had then already obtained translation rights for 'famous' chemistry texts, and that he contacted Klautz in order to give him guidance 'with the publication and printing (of these books).'²³ Thirty-five years later his associate, Johan Gerhard Nordemann, let slip that 'those books by Karrer, Richter-Anschütz and Feigl belonged to Dekker, who discovered them, and who was, even then, as clever and competent as he was untrustworthy.'²⁴

There is nothing of this to be found in the minutes of the board of Elsevier. We read only of an exiled German lawyer who, in 1936, offered the rights to publish scientific works that were forbidden in Germany, and about a trip that 'Mr Klautz made through Germany, Austria and Switzerland in order to familiarise himself with regard to the possibilities of selling books, published by us, in those countries.'²⁵ In a memorandum to the board a year later, Ted mentions that fiction writers who were 'no longer welcome' in the neighbouring country had found refuge with Amsterdam publishers such as Allert de Lange and

²³ According to his biography (see note 43), Dekker's contacts with Nordemann and Klautz developed as follows. Maurits Dekker (1899–1995) had studied microbiology and chemistry in Amsterdam. As a student he wrote columns for *Chemisch Weekblad* and *Pharmaceutisch Weekblad*, weekly journals that were published by Daniël Bernardus Centen. The success with these columns was such, 'that he became managing director of a new division of Centen's booktrade, a division for scientific publications. He also tutored. One of his students was Johan Gerhard Nordemann, who enlisted his help in teaching him the principles of chemistry. Nordemann wanted to use this knowledge in his summer beer enterprise and winter anthracite business. During the lessons they often talked of other entrepreneurial possibilities, which resulted in Nordemann's decision to become Centen's bookkeeper. When the owners of Centen wanted to retire, Dekker and Nordemann decided in 1927 to continue the company under the new name Dekker & Nordemann. The company grew very rapidly with the strategy of selling Dutch books by mail, specializing in the fields of chemistry and pharmacy. They had the foresight that English would become the common international language for science and technology, and thus decided to purchase the translation rights to famous German texts, such as those of Paul Karrer and Fritz Feigl. Since publishing was a different business than distributing, it was fortunate that Johan Klautz, president of Elsevier's publishing company, became interested in their enterprise. Klautz guided them through the production and printing process of their publishing programs.'

²⁴ Interview of Johan Gerhard Nordemann by Piet Bergmans in Nijmegen on 17 February 1971 [typescript in Bergman's archive]; Nordemann remembered that he was introduced to Dekker by the mathematician Jacques van Oss, author of *Warenkenis & Technologie* (books on consumer goods), and that both he and Klautz had gone to Rotterdam to say farewell to Dekker and his family when they sailed for America. 'Nordemann preferred to talk, however, about the brewery he had owned before putting his money in the business with Dekker. He proudly showed a painting of the brewery, and had me [Bergmans] leave through a book from 1903 where it was described.'

²⁵ *Elsevier 100* p. 133.

Querido very soon after the Nazis had come to power, but that writers of non-fiction had not yet done so. We quote:

With scientists the situation was different. For their publications, which were mostly the result of [prolonged] scientific work, the problem of finding a publisher arose not immediately but only in the last year [1936]. This concerns Jews living outside Germany who saw their contracts broken with their former German publishers, or else exiled opponents of the Nazi regime who would like to find a publisher outside Germany in order to avoid currency difficulties. Agencies have sprung up here and there in Europe whose job it is to interest publishers in the manuscripts of these exiled scientists. I saw highly promising manuscripts on *Massenpsychologie*, *Pharmakologie* and *Säuglingskrankheiten* (Children's diseases) and ascertained that the authors Aschwitz, Müller and Finkelstein were of world-wide renown. However, it is quite easy to conclude that there will be no sales of these books in Germany. Should we be able to make money from them, it would be a gift from heaven.²⁶

This was Ted's conclusion during the trip that we have already mentioned. He decided nevertheless to publish just one of these books, a handbook for paediatricians by Heinrich Finkelstein, 'a portly little gentleman with a head as round as a ball, a pronounced baby-face with two large bright blue eyes and a couple of hairs across his bald head.' He did so 'because this exceptionally engaging man had no interest whatever in the conditions, and only wanted the book to be reprinted and distributed as soon as possible.'²⁷ But he would turn out to be right. The bulk of it would be sold only when the war was over.

He came to his second conclusion—that he would need to obtain the rights to science books in German in order to publish them in English—during his visit to the Akademische Verlagsgesellschaft in Leipzig. It was he, not Maurtis Dekker, who bought the rights here for Fritz Feigl's *Qualitative Analyse mit Hilfe von Tüpfelreaktionen* (*Qualitative Analysis by Spot Tests*), a thick 'cook book' essential to any chemical laboratory.²⁸ He also obtained rights to the *Little Beilstein*, as the *Chemie der Kohlenstoffverbindungen* (*The Chemistry of Carbon Compounds*) was called. (The *Beilstein* itself will be discussed later.) This four-part work originated from the *Chemie der Fettkörper*, which Viktor von Richter had already

²⁶ *Elsevier* 100 pp. 135–136.

²⁷ Klautz pp. 79–80.

²⁸ *Elsevier* 100 p. 142; mention of the acquisition of English rights on all three books (those of Feigl, Richter-Anschütz and Karrer). See also Klautz pp. 82–84.

described at the beginning of the century and was brought up to date by Richard Anschütz. The volumes on aliphatic compounds had already been published in English, but not the remaining two volumes, and this raised expectations because of the important aromatic and heterocyclic compounds discussed in them.²⁹ Kurt Jacoby sold these rights because he could get foreign currency for them, and for this reason Leo Jolowicz, his father-in-law and owner of the Akademische, immediately agreed to it.

Even though Ted may have actually carried out the transaction, we can assume that the suggestion came from Maurits. To obtain the rights for *Lehrbuch der organischen Chemie* (*Organic Chemistry*) by the above-mentioned Paul Karrer, he needed to go to Georg Thieme.³⁰ Thieme was also in Leipzig at the time and quickly relinquished the rights, also due to currency problems. As it turned out one year later, Ted was fortunate with his purchase as no sooner had his English version appeared on the market in 1938, then the writer was awarded the Nobel Prize for chemistry. Undoubtedly Paul Karrer, professor at the University of Zurich, who had succeeded in synthesising the natural colouring agents in carotenes and specific vitamins, had a right to the prize; however, he had to share it with a biochemist from Birmingham, Walter Haworth. *Karrer's* was used extensively in many universities, at least in mainland Europe, because it was so good. But now, demand for the English version would exceed all expectations. The significance of these purchases for Elsevier will be discussed later. We just mention the material consequences involved in the publication of chemistry books on the international market, such as the fact that Dirk Meijer of Wormerveer, who printed for Elsevier, had to invest heavily in moulds to compose chemical formulae, train his staff in this special typesetting, and have them learn English.³¹

²⁹ Ibidem.

³⁰ Ibidem.

³¹ Klautz p. 84, also p. 107; *Elsevier 100* p. 143; *Chronicle* p. 14: 'Dirk Meijer [...] smelled the future in the texts Klautz wanted to publish. He became Klautz' ally, invested in moulds and instructed his type-setters—had them learn English, for instance. In this way he conquered a monopolistic position among Dutch printers. The immediate future, with its threat of war, might not be rosy, but Meijer believed that in five or ten year's time he would be in business.'

Encyclopaedia of Organic Chemistry

To revise existing works is one thing, to create new ones another. Now that ‘the competent’ Maurits Dekker had treated him to a glimpse of science publications, the ambitious Ted Klautz wanted something new to show for himself. But what? Even without the professional knowledge of a Dekker or a Jacoby he could see that the synthesis and application of organic chemicals owed their great success to the enormous diversity of carbon compounds, and to the on-going unravelling of their structures—an intellectual fascination in itself. Developments in this field were so compelling that no science publisher could ignore them. But if ‘everyone’ was publishing works in organic chemistry, what could Elsevier—with no name in this field—come up with? Probably he asked Kurt Jacoby for advice, for it can be no coincidence that while visiting the Akademische he was put in touch with a former staff member who had worked on *Beilsteins Handbuch der organischen Chemie*.³² Ferdinand Springer, who published this prestigious handbook, had been forced to dismiss her because she was Jewish, after which she had found temporary work at the Akademische. Her name was Edith Josephy.

The idea of *Elsevier’s Encyclopaedia of Organic Chemistry* must have come from her. We don’t know for sure, but it cannot be otherwise. According to Stefan Radt, who had known this enthusiastic woman well, she was full of ideas for just such a project.³³ To him she was

³² *Elsevier 100* pp. 146–147: ‘It was Nordemann who brought Klautz in contact with Jacoby of the Akademische Verlagsgesellschaft. This man inspired Klautz to what had to become the masterpiece of Elsevier’s science list, an English encyclopaedia of organic chemistry that could replace the German *Beilstein*.’ See also Klautz p. 111: ‘Jacoby [...] enabled a visit of Miss Josephy [together with Radt] to Elsevier in Amsterdam, to discuss the project of a short, critical and conveniently arranged encyclopaedia of organic chemistry.’ A letter of 1 April 1975 from Maurits Dekker to Piet Bergmans [typescript in Bergman’s archive] adds some detail: ‘In September and October 1937, Dekker was in the U.S., to assist Proskauer, manager of the Nordemann Publishing Company [in the U.S.], in setting up a distribution system. In the meantime, due to the almost continuous contact between the Elsevier director and Nordemann [in Amsterdam], the former became interested in a plan of Jacoby to start a new *Beilstein*. A meeting was held in the Hotel Adlon in Berlin, arranged by Jacoby, where from the Dutch side Klautz and Dekker were present and where from the German side, in addition to Jacoby, there were Josephy and Radt. The conclusions were positive, and as a result both chemists came to live in Bussum, Holland, in order to be in close contact with Dekker, who lived there too.’

³³ Interview of Stefan Radt on 3 April 2003. [Stefan, Fritz’ son, became professor of Ancient Greek in Groningen.] In his letter of 1 April 1975, Maurits Dekker writes to Piet Bergmans: ‘Of the two editors of *Elsevier’s Encyclopaedia of Organic Chemistry*,

'Aunt Edith'. His father, Fritz Radt, would still have spoken of her with deep respect—they had been colleagues for more than ten years, up until the time that she was abducted by the Nazis and murdered. No doubt she would have suggested to him the idea of publishing an *Encyclopaedia* in English—with the new aim of classifying organic compounds according to their molecular structure—when she met him in Leipzig in 1936. It could compete with the fourth edition of *Beilstein*, because this had not got anywhere near as far as the description of the tri-, tetra- and pentacyclic compounds (compounds with three or more benzene rings that were frequently used in the medical world). The editors, who included Fritz Radt, Dora Stern and Friedrich Richter, had already considered a similar new format for the fourth edition in 1935. Shortly after the first two were dismissed, for the same reason as Edith Josephy, and Friedrich Richter succeeded Bernhard Prager, the Jewish general editor who Ferdinand Springer had already been forced to dismiss in 1933.³⁴

Ted jumped at the idea. Without stopping to consider what such a publication would entail—'he needed only to smell it'—he arranged emigration for Edith Josephy, as well as for the jobless Fritz Radt, who she had recommended. He bought them both a house in Naarden, a suburb on the railway line into Amsterdam, and had them quickly work out a plan for *Elsevier's Encyclopaedia of Organic Chemistry* at the publishing house on the Nieuwezijds Voorburgwal in Amsterdam.³⁵ By 1937 he knew that the work would consist of eighteen volumes, but as far as he was concerned, more would not be a problem. Chemists would have to be appointed, a dozen, maybe more, and office space would be needed, possibly a whole property. He made no budget, believing that sufficient funds were available. The fifth edition of the *Winkler Prins* was selling well; *Rubber* was producing plenty of cash. And wasn't there a new bestseller on the way?—a novel about sea voyages, *Hollands*

Edith Josephy proved to be the most dynamic, so that when she was abducted by the Germans, the main effort of the encyclopaedia collapsed.'

³⁴ Sarkowski p. 336.

³⁵ Klautz p. 111: 'The undertaking would take millions. The initial investment alone required many hundred thousand guilders. We had to buy costly back volumes of chemical journals, and to pay at least two or three year salaries for dr Josephy, dr Radt and a few highly qualified collaborators.' In *Chronicle* p. 10, it is argued [probably by Bergmans] that no budget was fixed, because at that time no one could estimate how fast the chemical literature would grow, but that this recklessness laid the foundation of Elsevier's competence as a science publisher: 'When a budget had been made, one would never have started such an enormous project.'

Glorie by Jan de Hartog, which would make a small fortune with an expected sale of more than 100,000 copies. Herman Robbers, now chairman of Elsevier's board, fully supported the plan to publish the *Encyclopaedia*, and even encouraged him: 'Anything that can damage the Hitler clique is a blessing for humanity.'³⁶ To understand what he had embarked on, let us take a look at the prestigious work that he intended to overshadow.

The *Handbuch der organischen Chemie* was the lifework of Friedrich Beilstein, at one time professor at the Technological Institute of St. Petersburg.³⁷ In 1860 he started to make an inventory of the components of petroleum, and in 1880 he had enough material to fill a book. The fame of his *Handbuch* was due not so much to its completeness—because even then new chemicals were being discovered all the time—but to the fact that there was hardly a mistake to be found in it. By the end of the nineteenth century this assiduous man had managed to produce three editions almost entirely on his own, with data on respectively 20,000, 40,000 and 70,000 compounds. However, after that he could no longer cope with such numbers. Shortly after the turn of the century a team was needed (a team that worked for the German Chemical Association) to supplement the third edition, after which the set-up had to be changed as well. Springer-Verlag started publication of the revised fourth edition, under the leadership of Bernhard Prager, in 1918. That it would take on gigantic proportions was clear from the very beginning, when the description of the derivatives of ethanol required 22 pages, instead of the seven in the third edition, those of acetylene 16 instead of two, and those of prussic acid and its salts 60 instead of 24.³⁸ In 1936 the fourth edition consisted of 20,000 pages on 200,000 compounds. And as we have said, it was still far from complete. At the time there were two teams at work, one for the literature from 1910 until 1920, and another for the literature from 1920 until 1930. While an overview of the literature was said to be almost complete, already a second series of additions was being worked on...

The two scientists, Edith Josephy and Fritz Radt, who Ted had brought over to Amsterdam in the beginning of 1937, did not wait for more staff to be appointed. In fact, it was not until after the war

³⁶ *Elsevier 100* p. 147 (*Chronicle* p. 10).

³⁷ Richter pp. 61–84.

³⁸ Richter p. 92.

had ended that a team of twelve chemists and twenty assistants could be formed—still a modest number for a reference work of eighteen volumes that had to be completed in ten years time. Helped only by a couple of students, they started work immediately on revising the material on tetracyclic compounds that had already been compiled at Springer (with all the literature up until 1937; here *Beilstein* was 18 years behind). The volume of *Elsevier's Encyclopaedia of Organic Chemistry* in which these were described was the first to be completed, but was numbered as volume 14. Correction of the 736 pages of formulae with Kékulé structures took up a great deal of time, although Meijer's setters in Wormerveer had done their best. But in March 1940 they had made sufficient headway, and 4000 copies were printed. The loose sheets awaited binding, but two months later war broke out.³⁹

Prior to this Ted had set up subsidiary companies of Elsevier in London and New York, to promote the development of his science publications in English—an unheard of step in those times.⁴⁰ It is clear that he had in mind first and foremost the *Encyclopaedia*, and the unpaid-for titles (such as *Karrer*), as this work is explicitly mentioned in the contracts. The London subsidiary was established in May 1939, and received a symbolic deposit of 100 pounds; the subsidiary in New York was ready in January 1940, with a rather more substantial 50,000 dollars.⁴¹ In London Elmer Bates, director of the Imperial

³⁹ Josephy & Radt, *passim*; *Elsevier 100* p. 148: 'The two editors wrote the 20 introductory pages in March 1940, Meijer had everything printed in April, and when in May the war broke out the printed sheets were hidden by the binder Proost & Brandt'; Klautz p. 112: 'No doubt the Germans would immediately seize this English competitor of a German monopoly that was composed by Jews.'

⁴⁰ *Chronicle* p. 13 and p. 16: 'Elmer Bates in London may have been a friend of a friend of Klautz' tennis friend'; see also *Chronicle* pp. 7–8 on 'the American adventure'.

⁴¹ *Elsevier 100* p. 144: '[In 1939] Klautz proposed to found an Elsevier Publishing Company, Inc., in New York, with a capital of 100,000 dollar, of which 75,000 is deposited: 50,000 by Elsevier in Amsterdam and 25,000 in shares (5,000 by Nordemann Publishing Company, Inc., and 20,000 if possible by third parties).' Much more detail is given in *Chronicle* pp. 27–31, where obviously use has been made of correspondence by Bergmans. In the letter of 1 April 1975 from Dekker to Bergmans, quoted in note 29, Dekker also writes that Klautz had approached Pierson in Amsterdam for the financing of his publications in America, and that the reply was: 'You, Elsevier, had to reduce the value of your shares by 2/3 only a few years ago. Therefore, we cannot advance any money to you. But if the young chemist Dekker is willing to go to the U.S. for you, we are willing to give him 300,000 dollar to found a world Elsevier in New York.' Bergmans reports this to Klautz on 12 May 1975 [typescript in Bergmans' archive] and asks whether Dekker's story is true. Klautz' answer is unknown. Bakels' draft history quotes minutes of a board meeting (*Elsevier 100* p. 146): 'Pierson was immediately willing to take part, but didn't want to become the banker of the Incorporated [in

Book Company, acted as executive secretary, and in New York Maurits Dekker was given the job. This last comes to us as no surprise—by now Maurits had become a good friend, just like Erich Proskauer. Proskauer had worked as editor at the Akademische in Leipzig and had called on Ted two years before, after he had managed to leave Nazi Germany, to impress upon him the absolute necessity of having a branch abroad. He had received some money from Johan Nordemann and was in the process of setting up a publishing company himself in New York.⁴² Eric (for that is how he now spelled his name) now made an offer to Ted to take a 10% share in the American subsidiary of Elsevier, and also promised editing work to Maurits. From November 1939 it was Maurits with whom he would deal. Already in May—the month the London subsidiary of Elsevier was established—Ted had urged Maurits to take the boat to safety, but he procrastinated. Should he go alone, or take his wife and children? They were Jewish, and the question was whether Jews in the Netherlands would become ‘no longer welcome’ just as in Germany, should the war-hungry Nazis succeed in occupying the country. When they finally took the plunge and the whole family was ready to emigrate, it was already September; England had declared war on Germany and mines were laid in the Channel. ‘The *Zaandam*, in which we sailed over in safety, was the last but one of the regular

New York] as long as Twentsche Bank was the financier of the mother-company. It was decided to take Pierson as banker instead of Twentsche Bank.’ Here, the loan (50,000 dollar?) isn’t specified.

⁴² *Chronicle* pp. 5–7; some details are given by Eric Proskauer in his letter of 27 February 1975 to Piet Bergmans: ‘As Dekker and Nordemann did not have the necessary financial means to sustain a publishing house in the U.S. on its own, they went to Klautz and suggested him to start an Elsevier Scientific [in New York] to salvage their American venture. An agreement was made between Elsevier and Nordemann Publishing Co. Inc. which provided that I [Proskauer] would act as American editor for Elsevier and sign contracts with authors for them. [At the same time, in 1937, Nordemann established a contact of Josephy and Radt with Elsevier.] In 1938 I visited Klautz in Holland, and it was decided then to start an Elsevier Publishing Co. in New York, for which purpose Elsevier would sell shares on the Amsterdam stock exchange. To implement this plan, Dekker came to New York in the spring of 1939, but he [returned soon] to Holland, since no Elsevier money had yet been transferred to the U.S.’ When after the war communications were restored, ‘it became clear that Elsevier Scientific Publishing Co. had indeed been founded, in 1940, and that its capital of 14,960 dollar—what remained of the 50,000 dollar loan at the moment Germany invaded Holland—was entirely the property of the mother company.’ (*Chronicle* p. 80) The American enterprise of Dekker and Proskauer is described by Hendrik Edelman in *Logos* **15** (2004) 188–193 and *Logos* **16** (2005) 41–47.

liners from Holland,' Maurits wrote. Referring to himself in the third person, he continues as follows:

From December 1939 until May 1940 Dekker received a salary from Elsevier and he managed the distribution of Elsevier books in the United States and Canada. It is around this time that he started to work with Proskauer. After Germany invaded Holland in May 1940, contact with Europe was no longer possible. So all property [what remained of the 50,000 dollars] was put into an escrow account, and a new publishing company was set up: Interscience Publishers Inc. Maurits Dekker became president and Eric Proskauer vice-president, and later chairman of the board of directors.⁴³

Ted knew nothing of all this. He didn't even get to see the memorandum of association of Elsevier Publishing Company Inc. in New York. Neither did he hear anything from the London branch, where anyway little had been invested. Elmer Bates could only sell the book stock, then wait and see what would happen next. For Ted the German attack came as no surprise. Hadn't he served in the mounted artillery? In August 1939, he was mobilised, and encamped with his regiment near Amersfoort.⁴⁴ Here, for months, he awaited the coming of the Germans, performing trial runs on a splendid grey, rather stiffly in the by now tight fitting uniform. On the second day of the war, 11 May 1940, he helped to set fire to barns and farms that lay in the line of fire, but the shooting never happened. Two days later, the order came to retreat to a fortress on a strip of land flooded as a defence line. While his canon was being dismantled, he was caught in machine-gun fire. Slipping hastily from his horse—by now a perfect target—his foot got stuck in the stirrup and he was dragged along the ground, for his grey had bolted. So fortunately for Ted, his baptism by fire ended only in bruises, not gunshot wounds, and after this he spent two weeks as a prisoner of war. Feeling rather subdued after the violence he had endured, he thought it wiser not to bind the loose pages of volume 14 of the *Encyclopaedia*, since this work was, after all, directed against the Nazis. So, as soon as he was released he had them concealed in a secret hiding place, then straightaway set about finding an office for his science editing work—an office that could not be connected with

⁴³ Marcel Dekker, In Memoriam Dr. Maurits Dekker, *Journal of Macromolecular Science* **A26** (1989) nr. 8 pp. 3–6 (here p. 5).

⁴⁴ Klautz pp. 90–91.

Elsevier. This he found on the Herengracht in Amsterdam, and because of the utter silence which reigned there it quickly became known as ‘the monastery’.⁴⁵

At the Nieuwezijds Voorburgwal Edith Josephy and Fritz Radt had already begun their description of the tricyclic compounds, and they finished it at the monastery. They were certainly helped, but we don’t know exactly when experienced chemists such as Jan van Alphen and Chris Kamminga, the future professor, came to their assistance. Probably they had reached as far as the bicyclic compounds by 1942. The thousands of publications on these compounds, some very recent, arrived for their inspection from the Academy of Sciences on the Kloveniers Burgwal. That year, however, they had to cope with measures imposed by the German occupier. In May Edith Josephy and Fritz Radt had to wear the yellow Jewish star, and in July were given notice to leave their place of residence, Naarden, and to report in Asterdorp, a camp surrounded entirely by water to the north of the river IJ, close to Amsterdam. Although they had heard of work camps in Germany, neither of them thought of going into hiding. ‘It didn’t occur to them that a government could be criminal’ according to Stefan Radt, who was able to tell us the following.⁴⁶ ‘Edith reported to Asterdorp, only to disappear from the face of the earth. She disappeared together with her elderly mother, after first making a rucksack for herself and her mother out of strong curtain material...but Fritz stayed at home, and had his doctor sign a certificate of serious ill health and would therefore be unable to undertake the journey. Bob Roelofs was prepared to bring to his house the books and journals that he was working on at the monastery, so that he could carry on with excerpting. Bob was a student who was assisting him, and was also in the Resistance. Each time he went to Naarden he pointed out how easy it would be to arrest someone who stayed in his own home...And now, since the disappearance of Edith, and the fact that the future of the *Encyclopaedia* depended upon Fritz’s welfare, Ted wanted something better than a doctor’s certificate. He therefore made a request to the authorities (the Department of Education, Science and Protection of Culture in the Hague) to release ‘his’

⁴⁵ Klautz p. 112; the name ‘monastery’ was mentioned by Yvonne Meijer-Praxmarer in the interview of 25 March 2003—see note 57; it was on a floor of a building with the name ‘The Trade’ (‘De Koophandel’) and had an extensive library (*Chronicle* p. 11).

⁴⁶ Interview of Stefan Radt on 3 April 2003; the story is summarised in Klautz pp. 112–113, and just mentioned in *Chronicle* p. 12.

Jew from deportation, due to his being indispensable. The Academy of Sciences, which was consulted on the matter, supported this request:

The encyclopaedic work on organic chemistry will, indeed, meet a very great need, for science as well as for industry. Without a doubt the editor possesses the exceptional and rare gifts that are required for a documentary work of this kind. Therefore it is in the public interest that Dr Radt be allowed to continue his work on the encyclopaedia undisturbed.⁴⁷

This would have been written to protect him against any disaster. But was Fritz Radt really so exceptional? According to his son he was a shy, cautious man, rather small and not particularly Jewish-looking.⁴⁸ He was born in 1893 in Berlin, where he studied medicine, biology and chemistry. He obtained his degree in the middle of the First World War, after which a small heart defect kept him from joining up. He went to work at the chemical laboratory of Böhringer-Mannheim, but the work did no justice to his uncompromising accuracy—a valuable quality for a researcher. In 1925 he returned to Berlin and found work with Friedrich Richter, who was working on the *Beilstein* for the German Chemical Society in the Bendlerstraße. Here he also found himself a wife, Julia Cohn, who gave birth to Stefan in 1927. None of this is particularly exceptional. It is, perhaps, only his accuracy which is so exceptional—so much so that the one editor of the *Encyclopaedia* we were still able to speak to immediately remarked on it.⁴⁹

The request by the Academy of Sciences to leave him to work undisturbed went unheeded, however. We have not seen the relevant official document, but this must have been the case, otherwise Ted would not have gone to Fritz in the Christmas of 1942 to beg him to go underground.⁵⁰ In fact, the time came one evening in February, after the police had warned of a *razzia* planned for that night, and Fritz fled his home, together with his wife and child. When they got as far as the house of an author from Elsevier where they had been expected, they

⁴⁷ Letter of 24 November 1942 from Martinus Woerdeman, secretary of the Academy of Sciences in Amsterdam, to the Science Ministry in The Hague; a letter of 13 November 1942 from the Ministry to the Academy refers to Elsevier's request that Dr Radt be exempted from deportation.

⁴⁸ See note 46 of this chapter.

⁴⁹ 'Radt was severe.' Yvonne Meijer-Praxmarer in the interview of 25 March 2003—see note 63. *Chronicle* p. 60: 'Radt was terrifyingly precise and could drive us to despair by requiring a fourth, fifth and sometimes even a sixth proof before he allowed that the sheet be printed.'

⁵⁰ See note 46 of this chapter.

ripped the yellow Jewish stars off their coats, then walked eastwards for two hours through dark woods, to a village where Ted was waiting for them, and he brought them to a house full of people in hiding. The following evening Bob Roelofs arrived, and took them on a walk of four or five hours to the south, once again through dark woods, until they arrived at the Soest Resistance hideout. Here they were allocated the upper storey of a house, but they had to remain inside and keep dead quiet. Bob brought them their most important belongings from Naarden in a kit bag, and once more Fritz got his work delivered. When news came that Bob had been arrested, they had to leave post-haste to a farm—in their hurry Stefan, the son, managed to hide away his father's notes in the rafters. Such a detail is important if we wish to know what editing the *Encyclopaedia* at that time actually meant, as well as the fact that this particular detail concerned notes on anthracene. In September, when false personal identity documents were ready, they were able to leave the farm for an apartment in the Valeriusstraat in Amsterdam, for which Ted paid the exorbitant rent of 300 guilders a month, without batting an eyelid.⁵¹ It was not far from the monastery, where the crates of books and journals for his excerpts were packed. But even at this expensive address he was not safe. When the rumour came that the secret office was discovered where their false personal identity documents were printed, they fled to a house on the Prinsengracht and stayed there for six weeks. In fact, Fritz lived for more than two years at the flat in the Valeriusstraat, until December 1945, as the house in Naarden was damaged by a nearby bomb explosion and had to be repaired.

The Nazis made editing (not to mention actual publication) of the *Encyclopaedia* an extremely difficult task; at the same time they put great pressure on Elsevier's other publications by such methods as intimidation, imposing limitations on paper allowances and bans on publication.⁵² A vexed Ted, who sabotaged what he could, and went

⁵¹ Interview of Stefan Radt on 3 April 2003: 'Klautz' story about the open coach with horse, by which my family was transferred to an apartment in Amsterdam, with a moment of danger when the coach was stopped by a German officer, is complete nonsense. I regret to say this, since he saved our life.' [Klautz pp. 113–114.]

⁵² *Elsevier 100* p. 244: 'Official inspection was severe, the governmental impediments were numerous, paper was despairingly scarce, hardly any allowance was given for publications, and our bestseller *Hollands Glorie* [a novel by Jan de Hartog] was prohibited. Yet there was a wonderful team spirit, and the Christmas of 1943 was celebrated in style and dignity.'

into hiding himself for a while after daringly escaping arrest by the Security Police,⁵³ had great plans for Elsevier, even well before the fall of Germany. There was to be a sixth edition of the *Winkler Prins* and *Elseviers Weekly Journal*, a new opinion-forming periodical intended as a continuation of the 'neutral and stimulating' monthly journal banned in 1941.^{54, 55} Ted would not be Ted if he did not also quickly succeed in scoring a bestseller immediately after the war, this time Churchill's memoirs, which were to be translated in Dutch by Floris Bakels⁵⁶ and were to appear in the period 1948–1954. But Ted had further plans. In Amsterdam during the war he had ample time to talk to chemists Roel Houwink, Jan Ketelaar, Hendrik Westenbrink and others, who liked to refresh his knowledge of their subject and to tell him about their research—in polymers, quantum chemistry and biochemistry—and naturally he had offered them his services as publisher.⁵⁷ They recommended that he take on professionals to deal systematically with the selection, production and financing of this kind of publication, so as to avoid costly mistakes. So, shortly after the war, he took on Willem Gaade, Piet Bergmans and Dolf van den Brink to take charge, respectively, of these jobs. Their backgrounds and contribution to Elsevier will be discussed at length further on, but as far as the history of the *Encyclopaedia* is concerned they have as yet no role to play.

⁵³ *Elsevier 100* pp. 238–239: 'In March 1943 John Robbers was arrested by the SD [security police] and transferred to the concentration camp Vught, where he was kept until November of that year. The reason was that Betty Schoolmeester, the Jewess who took care of Elsevier's administration, had given him some costly ornaments in custody—a fact she had to admit in the course of the interrogation after her arrest. The SD had arrested her in Elsevier's office in July 1942, together with three other, non-Jewish employees. Betty Schoolmeester never returned. As for [Johan] Klautz, a number of times he had a narrow escape. He was often interrogated by the SD about suspect practices, but always seemed to have a good argument. However, on a day, at dawn, the police rang at his front door. He didn't hesitate and jumped from the balcony at the back of his house in the garden and crawled in the chicken-house of his neighbours—a hiding that was more comfortably continued in a hotel near Elsevier's office, and then in Bergmans' bookshop in Tilburg. Later on, he had an alarm installed in his office, which the porter had to activate when the SD was in sight.'

⁵⁴ Klautz pp. 115–122(139); *Elsevier 100* pp. 247A–247E.

⁵⁵ List *passim*.

⁵⁶ Executive Secretary Floris Bakels not only translated Winston Churchill's *The Second World War* in Dutch [Klautz p. 162], but also wrote his own memoirs of the war: *Nacht und Nebel* [Bakels *passim*] about his role in the Resistance, his arrest and the horrors of the concentration camps he survived, a.o. Natzweiler-Stuthof (Alsace) and Dachau (Bavaria).

⁵⁷ Klautz p. 78; *Elsevier 100* p. 151: 'In order to know which findings were important enough to be published, it was necessary to keep in touch with the scientific world'.

How vital it became, now, for the project to really get going! Volume 14 was already bound, and was published in the beginning of 1946. In the same year they were also able to publish volume 13, on anthracene and the other tricyclic compounds. To speed up the editing of volume 12, Ted tried to get editors of the *Beilstein* over from Berlin where, amidst the ruins, they could not yet be put to work.⁵⁸ However, the Academy of Sciences put a spanner in the works, for in July, when the Ministry of Justice in The Hague asked them for advice regarding the immigration of four German citizens, they were told that:

... Elsevier's *Encyclopaedia* is of considerable value, but cannot replace Beilstein's *Handbuch*. It is in the interests of science that everything be done to maintain the still available apparatus for the continuation of Beilstein's *Handbuch*. To comply with the request of Elsevier's Publishing Company, however, the means to do so would be considerably diminished, while the interests of organic chemistry urgently demand the completion of this standard work.⁵⁹

Worse was to come the following autumn.⁶⁰ At the first post-war congress of the international association of chemists (IUPAC, the International Union for Pure and Applied Chemistry) that was held in London, the future of the *Beilstein* inevitably came up for discussion. The pirate edition that had been published during the war in America had received great acclaim, and German financiers there were keen for the so highly esteemed fourth edition to be completed. This is why Friedrich Richter, the editor-in-chief was invited to London to say what people there wished to hear: that, alas, during the war people had not been able to work on it, but that now they were diligently searching for new staff. When he had convinced the audience that the missing volumes would still be published, thunderous applause broke out. The hall was already emptying when Fritz stood up to present his alternative *Encyclopaedia*. Naturally, our man was there too, for this congress needed to be made aware that there was a handbook with both an improved classification and a more logical set-up. But Fritz was not the man to drag back an audience that was walking away. On the contrary, his soft voice, poor

⁵⁸ Letter of 22 July 1946 from the Science Ministry to the Academy of Sciences, in which it is asked to explain the scientific need of Elsevier's request that the Ministry of Justice concurs with the immigration of 4 German citizens [*Beilstein* collaborators].

⁵⁹ Letter of 3 September 1946 from the Academy of Sciences [Martinus Woerdenman] to the Science Ministry.

⁶⁰ *Chronicle* pp. 49–50.

English and superlative accuracy meant that practically no one was left by the time he arrived at his conclusions. A flop, according to Ted, who sat grimly by, for this was a unique opportunity to present Elsevier to the scientific world.⁶¹

Fritz's unconvincing performance said nothing, of course, about the quality of the work. The published volumes of 1946 were favourably reviewed—by Leo Ruzicka in a professional journal for the Europeans, and Louis Fieser for the Americans. Both were full of praise for the clarity, accessibility and comprehensiveness of these books.⁶² So Fritz and his staff quickly had to provide a supplement to volume 14 in order to keep up-to-date and stay ahead of *Beilstein*. By then the editing department needed re-organising, and Jan Meijer was appointed for the job. He was joined in 1947 by Yvonne Praxmarer from Zurich, an energetic young woman, warmly recommended by Ruzicka, the supervisor of her dissertation *Über ein serologisch inaktives polysaccharid aus kulturfiltraten von streptococcus equi*.⁶³ Here in the monastery Jan and Yvonne fell for one another, but such happiness did little to mitigate the pain of having to invest once again in a work that had not yet produced a penny. The

⁶¹ Klautz p. 114: 'Only in 1946 the *Encyclopaedia* could be presented [to an international meeting of chemists]. The reception was lukewarm, if not cold, since the work didn't cover the recent discoveries that were made in England and America [and had been unknown to Radt]. In short, the Germans had killed the project by their war (*sic*). Moreover, the difficult selling of volume 13 proved that people were accustomed to the *Beilstein*, and especially the older chemists didn't see the need of a new set-up.'

⁶² The reviews were by Louis Fieser (University of Cambridge, Mass.) in *Journal of the American Chemical Society* **70** (1948) 1294–1295, and by Leopold Ruzicka (Eidgenössische Technische Hochschule, Zürich) in *Experientia* **3** (1947) 38–39.

⁶³ Interview of Yvonne Meijer-Praxmarer on 25 March 2003. This remarkable woman was one of the first foreign scientists that came to work for Elsevier. She was born in Vienna, passed her childhood in the small town Mistek near Ostrava where her father had a factory, then went with her mother to Lausanne in Switzerland and finished there secondary school. This was in 1940, when she was 16. After a study in chemistry at the University of Lausanne, she moved to the Federal Technical Highschool (ETH) in Zürich in order to get a PhD under supervision of the Nobel Prize winner Leopold Ruzicka. In 1947, while she was in the final stage of her laboratory work, Piet Bergmans came to visit Ruzicka (the two knew each other from the time the latter had a professorship in Utrecht) and asked if he could recommend young chemists with a PhD to assist in the *Encyclopaedia of Organic Chemistry*. Although her thesis on the origin of a lethal horse disease wasn't yet written—it would take 4 years work in spare time to finish it—Yvonne seized the opportunity of a job and went to Amsterdam. She worked for Elsevier from 1947 to 1957: until 1953 for the *Encyclopaedia* and then for *Biochimica et Biophysica Acta* and *Analytica Chimica Acta*—Bergmans asked her to be 'mother' of these journals and make them profitable by accepting advertisements of analytical equipment (which after a few years was no more necessary). She left because the daughter she had together with Jan Meijer required her presence at home.

number of subscribers to the *Encyclopaedia*, only 700 in 1948, was way behind the thousands that Ted had expected. The annual turnover of the science publications (so not only the *Encyclopaedia*) that year was 200,000 guilders, and in the following two years 380,000 and 650,000, while annual outlay must have been close to a million.⁶⁴ Although his board of directors had not yet complained, Ted went in search of funds. In the summer of 1949, with the publication of a fifth volume (a supplement), he approached the Ministry of Education, Art and Sciences for a grant in the form of a guarantee to the value of seven hundred subscriptions.⁶⁵ This, he hoped, would enable him to break even, but he came away empty-handed. Once again it appeared that the Academy of Sciences had given a negative advice. Later, however, he was given the opportunity to explain why a guarantee of 700 subscriptions would be sufficient, and why the *Encyclopaedia* was actually better than the *Beilstein*. For this he took along his editor-in-chief, but Fritz returned from the meeting with his tail between his legs.⁶⁶ Professor Pieter Verkade, with whom they had spoken, informed them that in his opinion the state could not give a guarantee for a scientific work, and then lashed out with objections against the published volumes

... which are not unjustified. They concern the terminology, and especially the question of whether the literature is fully discussed. It is to be feared that without a preconceived and homogeneous system of terminology the usefulness of the handbook will turn out to be somewhat disappointing in the long run, as it becomes more complete. For this reason, as well, we cannot advise governmental support.⁶⁷

This was in the letter that the Academy wrote to the Ministry in November 1949. The argument ‘the usefulness will be... somewhat disappointing’ is so weak that there must have been more to it. It was probably this: at the time one crazy organic compound after another was put together under names that the makers thought up themselves, and Louis Fieser had praised the pragmatism with which the *Encyclo-*

⁶⁴ *Chronicle* p. 61.

⁶⁵ Klautz’ request of a state subsidy, in the form of a guarantee, is specified in a letter of 18 July 1949 from the Science Ministry to the Academy of Science, a letter in which the advice of the Academy is asked.

⁶⁶ See note 46 in this chapter.

⁶⁷ Letter of 27 October 1949 from the Academy of Science to the Science Ministry, with the advice to reject Klautz’ request; the advice was formulated by Pieter Verkade, Johan Wibaut and Fritz Kögl (Leopold Ruzicka’s successor in Utrecht).

paedia had gone along with all these names.⁶⁸ And what happened now? Pieter Verkade, the man who had acted as chairman of an international committee for the nomenclature in organic chemistry for 15 years, had found, in his official but by now untenable homogeneous system, the stick with which to beat the unfortunate Fritz Radt on the back.⁶⁹ How gruelling that in the *Beginselen der organisch-chemische nomenclatuur*, a book that the already mentioned Willem Gaade had published with Elsevier in 1948, Radt and Verkade should be thanked in one and the same breath for their advice and support!

It would be going too far to say that Verkade destroyed the *Encyclopaedia*, but he did nip it in the bud. The last person to get a chance to bring the project back to life was Piet Bergmans, who has been mentioned before. With a bit of flag-waving he managed to bring the number of subscribers to over a thousand, but not much and not enough, with the publication in 1950 of the first supplements on naphthalene (belonging to volume 12). Right up until the bitter end *Elsevier's Encyclopaedia of Organic Chemistry* just cost money. The end came in 1953, when Bergmans reached an agreement with Richter, behind Fritz's back, that Springer-Verlag would take over the uncompleted work from Elsevier.⁷⁰

⁶⁸ See note 62 of this chapter.

⁶⁹ Nye pp. 174–190 [Chapter by Bernadette Bensauode-Vincent]; see further Verkade, *passim*.

⁷⁰ *Personal Recollections* [by Piet Bergmans] pp. 5–13: 'No one in Elsevier believed that the *Encyclopaedia* was a viable project. We had to wait until 1952 before we had a small profit, but the effort to put each year a volume on the market was too much for us.' (p. 6). 'In 1953 and 1954 the turn-over of the *Encyclopaedia* was 176 thousand and 240 thousand guilders, 17% and 22% of our sales.' (p. 8). 'In April 1955, two years after my first contact with Tönjes Lange [of Springer-Verlag], we met in Heidelberg, where the selling of the *Encyclopaedia* could be discussed with Ferdinand Springer, in the presence of [the *Beilstein* editor] Friedrich Richter, Paul Hövell and his brother Otto Lange. Did I have to appear before a law-court? I was put on a simple chair in front of a long oak table and faced five serious gentlemen in comfortable arm-chairs. *Herr Gott* Springer, who sat in the centre, began by bluntly stating that I better could have stayed in Amsterdam, since he didn't have any interest in buying the *Encyclopaedia*. I had the presence of mind to answer that I had come because I was invited, but that I was relieved to hear about his lack of interest, since a recent rise in the subscriptions and the surprising demand of the supplements to volume 14 had given us second thoughts about the selling. These things weren't true. I played high. We had a sham fight. In fact, Springer seriously considered buying the thing [in order to get rid of the competition].' (p. 10). 'In October 1955 I met Lange in Frankfurt in order to finalise the selling contract, under supervision of two commissioners of our mother company, [Frits] Boot and [Dolf] Van den Brink. The latter annoyed Lange with his theory of the financing of encyclopaedias, but didn't realize that the *Winkler Prins* was something different than the *Encyclopaedia of Organic Chemistry*, which is a scientific work. Lange didn't need advice on international science publishing! I could see how his irritation

He even managed to obtain 216,000 guilders to do so, by letting them think in Heidelberg that Amsterdam had not yet decided to sell. The 16 supplements that were ready would be added to the *Beilstein*; this was the idea. Fritz Radt was then 60, and although there would be no question of integration with the *Beilstein*, Richter, his former employer who he had been forced to leave in 1937 but with whom he was still on good terms, asked him to edit four more supplements. These would be published in 1959, 1962, 1965 and 1969 by Springer-Verlag with the imprint Elsevier.⁷¹ Dora Stern, his former colleague who had survived the war and everything else, would help him out, and his pension would come from a fund for *Wiedergutmachung* (reparations)...

Little Beilstein

But let us return to 1946, the year in which Ted ceased to be directly responsible for Elsevier's science publications, and handed over the task of building up this publishing list to that learned gentleman Willem Gaade and the amiable, but slightly authoritarian Piet Bergmans. Further on we shall meet the colossus who he brought in to sort out the finances, Dolf van den Brink, who would have him removed from his own creation eight years later. But first we introduce the duo Gaade–Bergmans, who together made Elsevier science publishers into the success that we know today.

Willem Gaade was born in 1908 in The Hague, and Henri Pieter Maria 'Piet' Bergmans that same year in Tilburg. Willem Gaade 'a gentle giant who spoke slowly, with big moist eyes'⁷² studied chemistry in Leiden, and did a PhD on the *Derivaten van Aethyleen-1:2-Dioxaminezuur* (*Derivatives of Ethylene-1:2-Dioxane Acid*) (90 sorts!), and then went to work at the Rubber Foundation in Delft under Roel Houwink. Piet Bergmans, 'so ordinary-looking with his blue-grey eyes and severe parting in his

grew—the muscles of his jaw were swelling—and when Van den Brink came up with unusual and irrelevant demands, I started to defend Lange's position. It was the only time in my career of 25 years that I turned against my commissioner.' (p. 12). 'The final contract was signed in June 1956; we got 216 thousand guilders for the *Encyclopaedia*, more than the 100 thousand we had had in mind.' (p. 13). See also Götze p. 54.

⁷¹ See note 46 of this chapter.

⁷² Characterization by Marc Atkins, collaborator of Willem Gaade, on 16 August 2005.

short straight hair,⁷³ had inherited a bookshop in his hometown. He was young, still in his thirties, when he became a committee member of the Catholic Society of Booksellers. Thanks to his enthusiasm and a wide circle of acquaintances, he managed, even during the war, to mark up some successes, such as the advanced sale of the new *Winkler Prins*. It was this advanced sale that brought Piet into contact with Ted Klautz, while Willem met Ted indirectly, through a recommendation by Roel Houwink.

The two were supposed to work together, but it took years before they found a way of doing so. While Piet quickly realised that he was entirely dependent on Willem's knowledge, especially when it came to his nose for anything new in his own field, it was almost impossible to convince Willem that the splendid publications that he proposed must satisfy ugly economic rules of practice, rules that Piet knew like the back of his hand. In fact, they complemented one another admirably, and because they both recognised this, eventually they became that remarkable symbiosis of scientist and merchant.⁷⁴ But differences in personality also played a role. We quote Otto ter Haar, who succeeded Piet Bergmans as manager in 1972:

Gaade reminded me of Louis Couperus, the dandy from The Hague, who wrote sophisticated novels. He was married to a flamboyant concert pianist, Rucky van Mill, but she ran off with an English lover. Relieved of family duties, Willem filled the lonely night hours preparing his own texts for press. His personal life did not go unnoticed, but this is hardly relevant here. Bergmans, who had ventured with his large Catholic family all the way from provincial Tilburg to 'the worldly north' knew about it, but remained diplomatic. At work he was by far the more dominant of the two.⁷⁵

Both travelled a great deal throughout Europe and North America, but sometimes even further, to Asia and Australia; Willem to lure in the authors, Piet to negotiate their contracts and to build up a sales network. Willem's travels are the more interesting of the two, and we give just a single quote from a letter written by Piet as illustration of his exertions. The following Kafka-like account is addressed to a member of the publishing staff, and was written in a hotel room in New Delhi:

⁷³ Characterization by Rob Bergmans, one of Piet's sons, on 6 September 2005.

⁷⁴ *Chronicle* p. 54.

⁷⁵ Interview of Otto ter Haar on 11 April 2003.

If I'd known what was in store when I offered to discuss [a Hindi version of] *Techterm*... I have the feeling that you've tricked me into it... Yesterday, first of all, I had myself driven to the Ministry of Education. Here I was deposited in one of those little curtained-off cubicles (you know the ones), with an impressive-looking porter, who then requested a subordinate to go and find out where the Technical Division was. After about twenty minutes he returned with the news that he had found it, and that he would explain to my taxi driver where I should go. After an elaborate explanation the taxi driver roamed the city for about half an hour, then went back to the first address to ask the subordinate if he would join us so he could show us the way. The man was out but would be back 'any minute'. And, indeed, he did come back after a bit and accompanied us to 'Block M', an appalling place that made our shabbiest government offices seem like palaces in comparison. The corridors were full of cupboards with files lying on the floor, and in a room like yours there were at least ten people, with more files lining the walls, all tied up with filthy string, and no wrapping, of course. After what seemed like an endless number of corridors, fortunately still in the company of the subordinate, I found myself in front of a curtain (there are no doors here) behind which there was some discussion as to who would speak to me. You can imagine that by this time I had quite given up hope of finding the person who had received the copy of *Techterm*, let alone looked at it. After the discussion behind the curtain I was taken along more endless corridors, and finally shown into the presence of a gentleman, the 'one you want to see'—without introducing me and without any explanation. This gentleman looked visibly displeased to be disturbed without warning, so I hastened to give him my card and explain to him what I had come for. He turned out to be very pleasant and did his utmost to give a good impression of the Ministry, but he knew nothing of the matter. After I had shown him your letter he started telephoning, in perfect English, interrupted all the while by coolies carting in heavy files with a slip of paper that he was supposed to sign, which he promptly did, without even looking at them. Eventually he took me to the man without whose advice I wouldn't get a step further. We arrived at the room where he was supposed to be, but the coolie guard standing in front of it informed us that he was in a meeting and that it might still go on for about three hours. Thanks to the high rank of my guide, we managed to get him to come out for a minute to make an appointment for 4 o'clock today. Now, on my way I had noticed name plates above the many rooms and thought I'd be clever and take note of them...⁷⁶

⁷⁶ Letter of 20 November 1958 from Piet Bergmans, written in the Ashoka Hotel in New Delhi, to B. Schoute in Elsevier's office in Amsterdam [typescript in Bergmans' archive].

The letter is as long again and, as everyone had expected: *Techterm* never reached the man 'without whose advice not another step could be taken in the matter', the matter being a Hindi translation of technical terms in various fields. Wim Clason, an employee at Philips, had already arranged these terms alongside one another for five or six languages.⁷⁷ He was also compiling multi-lingual dictionaries for the different fields, and Piet smelt good marketing opportunities.

So to what lengths did Willem go in his exertions? We pass over those books that more or less just landed on his table, English versions of discussions on elastomers, colloids and chemical bonding, from his former boss Roel Houwink, the Utrecht professor Hugo Kruyt and the Amsterdam professor Jan Ketelaar, respectively.⁷⁸ Translations of books by a few German scientists also belong to this 'easily come-by' category, such as *New Atoms* by Otto Hahn and *Structural Chemistry* by Walter Hückel (the brother of Erich, the more well-known of the two, because he had been able to explain the double bond of carbon quantum mechanically, by distinguishing between σ - and π -electrons).⁷⁹ And what of the *Meyerhof Festschrift*? This was no more than a collection of essays on metabolism that students of the Nobel Prize winner Otto Meyerhof wished to offer their teacher on his retirement. But Willem made such a beautiful job of it that Ted considered it a pearl in his crown, had 20,000 prospectuses of it printed in both Amsterdam and New York, and didn't bat an eyelid when he sold only 1500 copies.⁸⁰ What we cannot pass over, however, is Willem's achievement in launching onto the market a completely new and greatly extended version of *The Chemistry of Carbon Compounds*.

The Chemistry of Carbon Compounds has already been mentioned, the translated version of an acquired four-volume German work, *Richter-Anschütz*, otherwise known as *Little Beilstein*. This version was published before the war, and reprinted during the war by Interscience in America,

⁷⁷ Willem Elbertus Clason first edited *Elsevier's Dictionary of Television* (1955), and went on by editing dictionaries of technical terms in *Electronics* (1957) and *Nucleonics* (1958).

⁷⁸ *Chronicle* p. 64 (Houwink and Kruyt) and p. 96 (Ketelaar). The translation of Houwink's book turned out to be problematic: 'Paul Edmonds, one of those well-bred editors of Cleaver-Hume Press in London, wrote that the number of English words was impressive, but that no Englishman would be able to understand what the author wanted to convey. It took two years before we had a readable edition. From then on we only employed translators who translated in their mother tongue.'

⁷⁹ *Chronicle* p. 63 (Hahn) and p. 62 (Hückel).

⁸⁰ *Chronicle* pp. 87–88.

but after the war it turned out to be practically unmarketable.⁸¹ Willem, who had spent years in the laboratory, knew why. It did not contain the newest discoveries; this was the only reason. The set-up was still superb: well-organised in its description of reactions, in its grouping of related bonds, and in the examples from organic laboratory practice. So when Piet was appointed, he immediately confronted him with his idea of bringing *Little Beilstein* up to date, without even giving him time to look into the profits of the old edition. But he didn't need to. With the flair of the merchant who has money at his disposal and sees something special come his way, he promptly thought of a plan. Elsevier must act fast, for someone else might have the same idea. It should again consist of four volumes, otherwise it would not be recognised, and because of the language it should be edited in England or America. The fact that, after the war, most of the knowledge on organic chemistry had also been brought together in these countries—something which could be assumed—served to make the choice even easier. It would be England, of course, because it was closer; contacts would be more convenient, and also cheaper. And, of course, it would be up to Willem to find a suitable editor.⁸²

In early 1947 Willem started out on his travels. All the chemists to whom he spoke found it a marvellous idea, but hastened to add that someone else should do the editing.⁸³ It is no longer possible to find out who they all were, but those who spring to mind are Walter Haworth, Ian Heilbron, Edward Hughes, John Ridd, William Waters and Herbert Watson—no, not Watson of Watson & Crick of the double helix still to be discovered, that was James. Great names...but Willem was undaunted, and in the end two of those mentioned would even go on to support his project. In the same period, he probably also paid a call on Cecil Wilson in Belfast, and on his cousin David Wilson in London,

⁸¹ *Chronicle* pp. 93–94.

⁸² *Chronicle* p. 94: 'Gaade was ordered to find an editor.' This wording (*Chronicle* p. 94) is almost certainly due to Bergmans, and unfair to Gaade, whose leading role in the realization of *The Chemistry of Carbon Compounds* is undisputed. Why then this ostensible instruction? The year before, when both Gaade and Bergmans started to work for Elsevier, Klautz had appointed first Gaade and then, one month later, Bergmans to be manager of the science publishing division. Since Klautz didn't tell Gaade that Bergmans would be the chief, not he, the relation between the two had been strained. 'Fortunately Gaade was clever enough to understand that Klautz had made a mistake and that the management of this division was a commercial, not a scientific affair.' (*Chronicle* p. 54)

⁸³ *Chronicle* p. 94: 'It soon turned out that the order was difficult.'

sadly the former the worse for liquor and the latter possessed of an illegible hand. But in neither case did this stand in the way of the editing of Willem's later book series, the *Comprehensive Analytical Chemistry* (begun in 1959, and still publishing today).⁸⁴ During these travels he must have started the legendary booklet in which he wrote who, in his opinion, would be awarded the Nobel Prize—legendary because he often turned out to be right. But good sense and intelligent argument were not enough to get what he wanted. Persistence was needed, as well as luck, and after two years of being sent from pillar to post, he spoke to Earnest Harry Rodd. Rodd was head of the colouring agents department of Imperial Chemical Industries in Manchester, was approaching retirement and rather liked the idea of using his free time to bring up-to-date the book that he had so often consulted while in the laboratory.⁸⁵ Later on he admitted that he had barely suspected what it would entail⁸⁶ when he signed the contract in July 1949.⁸⁷

For the first volume of part 1 of *The Chemistry of Carbon Compounds*, which was published in 1951, Rodd wrote a splendid historical chapter on the development of the structural chemistry of carbon. In fact, it was a splendid publication altogether with its blue binding, typography, and formulas, and Willem liked to joke that when he retired he would once more check all the formulas in *Rodd's*. He turned out to be the almost perfect desk editor, possessed with tact and ingenuity, who managed to steer all of his almost faultless titles through the production process, and therefore always later than planned. A patient diplomat, he also succeeded in placing the elite of British science on Rodd's advisory committee, first the laureate Robert Robinson from Oxford, the man of alkaloids, then very soon after that, the man of nucleotides, Alexander Todd from Cambridge. But how mistaken Willem turned

⁸⁴ Interview of Marc Atkins on 16 August 2005.

⁸⁵ Rodd—the Preface of the second edition in 1964 still mentions dr E.H. Rodd FCGI FRIC in Bath as adviser and calls him 'father of the present edition, who continues to act in a very practical sense.'

⁸⁶ Rodd—the Cumulative Index, published in 2000, is introduced by M. Sainsbury, who writes: 'When E.H. Rodd agreed to become the first editor of *The Chemistry of Carbon Compounds*, he remarked that he did so with diffidence, and undertook a task the magnitude of which he hardly grasped.'

⁸⁷ *Chronicle* p. 94: 'Since Rodd would only retire from I.C.I. in 1950, he needed permission from his superiors to immediately start his work as editor. It took some time before he got the permission, so that the contract was only signed in July 1949.'

out to be with the size of the work!⁸⁸ In 1949 he still thought that each volume would be 800 pages, and that a volume could be published every 6 months: so completion by 1952. But volume one would need 1462 pages and two parts, because Rodd couldn't manage it with less than 34 chapters and 66 authors. So understandably, the fourth and last volume could only be published in complete form in 1962; with extra text and supplements there were 30 parts in total. This was no longer a *Little Beilstein*! [A second edition, now under the editorship of Samuel Coffey, would be published in 1964, and would have 50 parts.] *The Chemistry of Carbon Compounds* would be a learning process, also for Piet. Rodd would have to be paid his annual 400 pounds for ten more years than originally planned, to recruit authors, spur them on and correct their work; the circulation of 5000 copies first aimed at would have to be considerably reduced, and impatient buyers be told time and again that quality went before speed.⁸⁹ But the *Rodd* got a warm welcome in the bookshops of Western Europe! And here alone Piet managed to dispose of 800 sets. We don't know how many were eventually sold, but estimate around 2500 sets. It would be Elsevier's first science publication to make a profit.

Book of Health

Now that we have come to the business side of publishing, it is high time we lived up to our promise and said something about Dolf van den Brink. 'That deep voice of the man discouraged all opposition.'⁹⁰ Rudolph Engelbert Marie van den Brink was born in 1919, in the smart

⁸⁸ *Chronicle* p. 95; the overlapping text in *Elsevier 100* p. 168, due to Bakels, mentions *The Chemistry of Carbon Compounds* as Gaade's *chef d'oeuvre* (master piece).

⁸⁹ *Chronicle* p. 95: 'At long last [in 1951] Bergmans got the opportunity to travel around in Europe and visit booksellers, who were very interested in projects such as *The Rodd*.'

⁹⁰ Interview of Otto ter Haar on 11 April 2003. Ter Haar added: 'Dolf van den Brink spoke with ease. He was as good in French as he was poor in English, and this worked against him at the end of his career. As an entrepreneur he had little merit, but he was a genius in making Elsevier rich. This bear, who in 8 years time managed to remove Ted Klautz from his own creation, was feared. Surrounded by weak, obedient characters such as Frits Boot, the delegate commissioner who was nominally Elsevier's second man, he aroused aversion all around. This came to the fore in the vote against his presidency of the [Dutch] Union of Booksellers, an affront he took with stiff upper lip.'

suburban village of Laren (not far from Naarden), into a pious Roman Catholic family with a Protestant distaste for idleness.⁹¹ His father had a carpet factory, and his older brother by four years was already a professor of economics when, in 1945, he, Dolf, joined Ted Klautz as financial assistant. He had got to know Ted through the Resistance in the last year of the war, but before that had lived in the shadow of his successful older brother in Tilburg. Here, at the Catholic Institute of Economics he was instilled with the concept of 'velocity of capital,' and at Piet Bergman's Catholic bookshop he happily immersed himself in 'the world of information media'. One day he would combine this concept and this world. When later he was taunted for not being a real publisher, he described the different faces that a publisher may have, not without humour, and therefore also somewhat biting about

...the exploiter. This is the publisher, who practices his trade for honest money.⁹²

But this is what he himself was like. From the moment he was entrusted with the management of Elsevier's financial assets, only one question interested him: how do I get a grip on the money flow?⁹³ He was like a lockkeeper who shuts and opens gates, so that capital can flow and information rises to higher levels. So he divided the company into departments, with science publishing made separate from the general publishing of fiction and the *Winkler Prins*, and from the 'Bonaventura' company, which published *Elsevier's Weekly*.⁹⁴ He set up a central administration, with multiple channels to the departments, and personally audited the most important results. He knew them by heart. After Mass, and when he had attended to his white peacocks that he was breeding for a St. John's procession, there was nothing more to keep him from ferreting through the velocity of capital of the various departments. He always had at his disposal the very latest figures from the head of central administration, who also pointed out any irregularities or leakage they might contain. There was always some mistake to be found. And woe to whoever was responsible. These were the methods of

⁹¹ Brink (*De uitgever*) pp. 7–9.

⁹² Brink (*De uitgever*) p. 24.

⁹³ Brink (*Economische structuur*) passim; a doctor's thesis, written when Van den Brink had retired as chairman of the board of Elsevier, with an authoritative overview of the structure and development of publishing enterprises in the Netherlands during the years he was active in the field.

⁹⁴ See note 88 of this chapter.

the seventeenth-century minister Jean-Baptiste Colbert, who brought welfare to the citizens of France, with his *dictature du travail*.⁹⁵ And sure enough, Elsevier became a rich and flourishing company...and Dolf became as feared as Colbert. ‘Unfortunately he developed the habit of abusing staff in public, if there was something they didn’t know, or if they’d made a mistake,’⁹⁶ and even those outside the company were not exempt from this habit of public execution. Meanwhile the board of Elsevier saw the profits soar, and in 1947 appointed him deputy director. The following year his gifted brother became Minister of Economic Affairs.

We have already alluded to the special status that science publishing has come to enjoy within Elsevier. Informally this existed from the moment Piet Bergmans took over in 1946, but it took five years before it was formally established. This is because Ted only found it necessary to set up a separate limited company when he embarked on his American venture—a venture that will be discussed later—wishing to prevent Dolf in Amsterdam from largely economizing science publishing out of existence. By putting a stop to the *Encyclopaedia of Organic Chemistry* leak, for example. ‘It’s a different atmosphere over there [than elsewhere in the company] and stringent restraints are, therefore, not desirable’. This is how he defended himself before the board, in his latent difference of opinion with Dolf with regard to ‘the liquidity question’.⁹⁷ Finally, on 17 May 1951 the time was ripe, and Elseviers Wetenschappelijke Uitgeverij N.V. was established.⁹⁸ Strangely enough, the word ‘Science’ was left out of the English name Elsevier Publishing Company. The notary laid down that the company was established in Amsterdam, and received a capital of 1 million guilders, consisting of a thousand shares of one thousand guilders, of which 20% was to be paid up in full and made available in cash. H.P.M. Bergmans was appointed Managing Director, and J.P. Klautz and R.E.M. van den Brink, who were directors of N.V. Uitgeversmaatschappij Elsevier (Elsevier Publishing Company Ltd.), became commissioners.

It still remained to be seen whether the daughter company could stand on its own feet, but the holding company was already a matron. It had outgrown its premises in the Nieuwezijds Voorburgwal, and added

⁹⁵ Prosper Boissonnade *passim*.

⁹⁶ See note 90 of this chapter.

⁹⁷ *Chronicle* p. 104.

⁹⁸ *Elsevier 100* p. 164; *Chronicle* pp. 88–89.

a large property on the Museum Square—in the force field between the Concertgebouw and the Rijksmuseum—only to commission the building of entirely new, imposing office premises in the Spuistraat, with fourteen bays and five floors that would cost 713,000 guilders.⁹⁹ It was Ted's heyday, and at Museum Square he received such foreign celebrities as the writer John Boyton Priestley with his 'flashily dressed' *grande dame*, crown pretender Archduke Karl von Habsburg, and Jim, son of the deceased President Roosevelt. He had the journalist Lodewijk Arntzenius play the piano, or else Stephen Bergmann who accompanied soprano Daisy von Saher with such sensitivity. He frequently counted amongst his guests former ambassador Mauritz van Vollenhoven, as well as Professor Joost van Hamel, president of the Special Court of Justice and author, and Eppo Doeve, illustrator of *Elsevier's Weekly*. But science was never represented, and in spite of his splendid cocktail parties, Ted felt a strange emptiness. It was as if, having succeeded so well in becoming a business man, he had lost his soul. This is how he wrote about it, later, also about his silent aversion to the 'fancy' Cadillac and chauffeur—which he had personally requested.¹⁰⁰ He had Elsevier acquire a teak yacht, complete with Bermuda rigging, and christened it 'Bonaventura'. It was meant for contemplative sailing trips on the IJsselmeer, but there was no time. Eventually he had it towed from Muiden to IJmuiden, where people sailed out onto the open sea, not the inland waters. From here, at least, he could make his getaway if the Russians...¹⁰¹

His main market was in New York, and in the continent beyond, where he expected to find the capital for a great worldwide science publishing house. He had already thought of emigrating to America, even before there was any question of new premises in Amsterdam. The idea was born on that day in February 1946, when Maurits Dekker met him off the boat, and in the dazzling night life of Times Square he bought a new pair of shoes, discarding his old worn-out European ones

⁹⁹ *Chronicle* pp. 98–99; Klautz pp. 166–167.

¹⁰⁰ Klautz pp. 168–169 and 171 (Cadillac).

¹⁰¹ Klautz p. 148: 'Western Europe was completely unprotected, the Russians needed only a handful soldiers to occupy Rotterdam, Antwerp, Paris... and there was no one to stop them. Russian deserters told that Stalin seriously considered adding the remaining part of Germany, The Netherlands, Belgium and France to Poland and Czechoslovakia.' [These exaggerations betray a fear of another occupation, a fear that was shared by many others and lead in 1949 to the North Atlantic Treaty Organization.]

beneath the magical illuminated advertisements.¹⁰² But already on that first visit it was clear to him that there was no money to be had from the man who greeted him at the pier, his old partner whom, he thought, he had lent a helping hand just before the war with 50,000 dollars for an Elsevier Publishing Company Inc. in New York. As we mentioned earlier, 15,000 dollars still remained of the original capital that Ted had given him to set up Interscience. Maurits had left this intact. He recounted to Ted the financial difficulties he and Eric Proskauer had encountered while trying to set up their own publishing company, and had so convinced his guest from Amsterdam of his worries, that Ted never raised the matter of the proceeds from the 35,000 dollars he had already used up, but signed, instead, an unbelievably generous contract for a five-year sole agency.¹⁰³ The discount laid down in it (40% for Interscience plus 40% for a sales organisation set up and managed by Maurits) meant that Elsevier had to add money on the sale of its books. So no more than 9.50 guilders went to Amsterdam for every copy of *Karrer's* that was sold in New York for (converted) 47.50 guilders, while the cost price was 8.90 guilders, and the author had a right to 12% or 5.70 guilders royalty. So 5.10 guilders had to be added.¹⁰⁴ Dolf van den Brink spotted this leak straightaway. If Interscience had been able to produce well-known American authors for Elsevier, the contract might have been defensible, but it was not to be expected that Eric Proskauer, outstanding editor that he was, would hand over authors instead of drawing them into Interscience. Let us clarify this by examining his relationship with Herman Mark.

Together with Hermann Staudinger, Mark was one of the two great men who discovered and described the macromolecules/polymers in rubber and cellulose, and paved the way for the spectacular growth of industries in synthetic materials, such as I.G. Farben and DuPont de

¹⁰² Klautz p. 146; see also *Chronicle* pp. 44–45: 'A telegram from Houwink in America had urged Klautz to come over as soon as possible, in order to make arrangements for the marketing of the 'Handbook' [*Encyclopaedia of Organic Chemistry*] in the U.S., in which the American Chemical Society might play a role.' [Dekker couldn't be of much help, his priority being the sales of Interscience: 'The propositions of Interscience had not been acceptable.']

¹⁰³ Klautz p. 147: 'The board of Interscience found the situation embarrassing, but I didn't want to see the money back, since the refugees had found themselves in an desperate position, in which I had probably done the same.'; *Chronicle* p. 89.

¹⁰⁴ *Chronicle* p. 85.

Nemours.¹⁰⁵ Staudinger never spoke of polymers, because the name said nothing about the length of the methylene chains they consist of—according to him these must be enormous and comprise at least 5,000 carbon atoms. Herman Mark never spoke of macromolecules, because initially he did not believe that chains of this length were a true entity—they had quarrelled over this in Germany around 1930. Mark was a Viennese Jew, who had discovered natural fibre structures in his pioneering work with X-ray diffraction at the Kaiser Wilhelm Institute in Berlin-Dahlem, before hearing that it would be better for him to move to I.G. Farben in Darmstadt, where Jews could still work undisturbed.¹⁰⁶ His relationship with Eric(h) Proskauer, as well as with Peter Debye, Paul Rosbaud, Hermann Staudinger, Heinrich Wieland and many others, even Albert Einstein, stems from this time in Germany. He really knew ‘everybody’. His professorship in Vienna, which began in 1932 when he was 37 years old, only lasted a short time, because the Nazi annexation of Austria forced him to emigrate to Hawkesbury, near Montreal in Canada, where he found employment in the pulp and paper industry.¹⁰⁷ Here, too, he showed himself to be highly competent in building up a network. A Hawkesbury contact at DuPont de Nemours, who was also a board member of the Polytechnic Institute in Brooklyn (New York), managed to have him appointed professor there in 1940. And almost immediately upon his arrival in Hawkesbury, he wrote to Eric Proskauer to come and discuss a series of monographs. The first volume of *High Polymers and Related Substances* was already in progress when Maurits Dekker was still in Amsterdam, and was produced in 1940, as the first publication of Interscience.¹⁰⁸ All these details serve to show that Interscience owes its rise in science publishing first and foremost to the work of Herman Mark. The very thought that a publisher would send an author like this to another publisher, or would want to, is absurd. Mark also writes in his autobiography of the difficulties he encountered in getting the *Journal of the American Chemical Society* to publish his articles on polymers, after which he ‘persuaded M. Dekker and E. Proskauer of Interscience to launch a *Polymer Bulletin*

¹⁰⁵ Reinhardt pp. 195–196 [Chapter by Peter J.T. Morris] and, in more detail, Krige & Pestre pp. 547–563 [Chapter by Yasu Furukawa]; Staudinger is usually given the credit of creating polymer industry on the basis of organic chemistry, despite his lack of physical insight into polymers: he, not Mark, got a Nobel Prize.

¹⁰⁶ Mark pp. 19–42.

¹⁰⁷ Mark pp. 83–91.

¹⁰⁸ Mark p. 91.

in 1945.¹⁰⁹ Having established it successfully, they then set up the *Journal of Polymer Science* in 1946, although at first there was little material, only enough for six thin issues per year. Even so, the first issue contained 57 articles. However, it soon became apparent that its quality must be greatly enhanced and publishing times halved, if it were to enjoy real prestige. It was Mark who put this into effect, and

...then we considered an American version of *Ullman's*. With the help of Dean Kirk and Donald Othmer, I persuaded Interscience to publish an *Encyclopaedia of Chemical Technology* to be edited by Kirk and Othmer... I became editor of the *Encyclopaedia of Polymer Science and Engineering*. This made millions for the publisher.¹¹⁰

Ted Klautz had already seen it coming, this succession of publications at Interscience, and by 1949 realised that Dekker, instead of being a figurehead in need of support, had now become a serious competitor. Each and every clause in the sole agency contract was a source of conflict. Dekker, for example, was against having his book series and his journal on polymers sold in Amsterdam.¹¹¹ If Ted wanted to get into the American market, then he'd have to split off from Interscience! By sheer coincidence, in that same year, he met up with an architect from Houston who had told him enthusiastically all about this boom town and its riches. We already know Ted as the man of intuition, and this is how he reveals himself once again. He went to Houston to have a look and, intoxicated by its bright colours, dazzling sunlight and sultry heat, the spacious houses 'that made Soestdijk (the Dutch royal palace) look like a little suburban home', and, he freely admitted, 'the good-looking, slim, sportily-dressed young women', he promptly decided that this was where Elsevier should be set up.¹¹² It was a jump in the dark, into an intellectual desert of oil refineries and their laboratories, with only one hospital and a few small institutes. His London representative had warned him that this was 'crying for the moon', but, even so, he straightaway started to attract local capital.¹¹³ His commissioners in Amsterdam only got to hear of it after he had set up Elsevier Press, Inc.

¹⁰⁹ Mark p. 127.

¹¹⁰ See note 109 of this chapter.

¹¹¹ *Chronicle* p. 86: 'On the request of Interscience, the name of the series *High Polymers* will be dropped [not be mentioned anymore in the Elsevier catalogue].'

¹¹² Klautz p. 194 (women) and p. 192 (moon).

¹¹³ See note 112 of this chapter.

in Houston.¹¹⁴ This must have been in 1950, because the contract with Interscience, which he wanted to get out of, expired in March 1951, although we do not know the exact date. Russell Cumley, a publisher of medical books, James Anderson, a rich cotton dealer, and two or more oil barons had lent him 40,000 dollars—who the barons were, we don't know. 'Everything had to be done quickly, there was no time for real planning, nothing was properly worked out, vital steps were omitted, with the result that mistakes were made. But this would only emerge later.'¹¹⁵ This we know from Floris Bakels, Ted's secretary, who managed to write down something of this adventure.

By the summer of 1951 it was abundantly clear that 40,000 dollars would not even be enough to launch Elsevier's existing English publications onto the American market, let alone new ones from American authors. Ted—who knew the liquidity problems only too well—could do little else than have the Amsterdam company make up the 85,000 dollar difference, and issue new shares.¹¹⁶ When his commissioners expressed their concern at these developments, he professed—all of a sudden calling himself president-director—to see a rosy future for Houston, unlike Amsterdam, where, as he put it, 'certain activities' could be continued, due to the low costs, and he found it absurd that the 85,000 dollars was recorded as credit.¹¹⁷ But as we are concerned here with science, not money, we shall not touch further upon the subject of the Houston finances. The outrage was, if anything, even greater when Ted summoned Willem Gaade to Houston.¹¹⁸ This was right in the beginning, in March, when the office was opened (in spacious premises on Lovett Boulevard, 204), and just six weeks before Elseviers Wetenschappelijke Uitgeverij N.V. was established. At the time Willem was in the middle of launching volume 1 of *Rodd*. The master ought not to be disturbed when in the middle of his first masterpiece, certainly not this master, with the promise of 'slim, good-looking, sportily-dressed young women'. Now he would have to cast about in that intellectual desert for scientific talent for Elsevier Press! Piet Bergmans, who saw his right-hand man depart, must have been furious. After sixty-seven days Willem was back

¹¹⁴ *Elsevier 100* p. 165; *Chronicle* p. 90; Klautz p. 199—the only words he spent on the matter are: 'The bond with Interscience was broken and Elsevier Press Inc. was founded, in Houston, with American shareholders.'

¹¹⁵ *Chronicle* p. 90.

¹¹⁶ *Chronicle* p. 98.

¹¹⁷ *Chronicle* p. 97.

¹¹⁸ *Elsevier 100* p. 166; *Chronicle* pp. 92–93.

in Amsterdam, however. ‘Did the cultural level—or lack of it—influence his decision?’ wonders Floris Bakels. Of course not, we say, he had to get back to *Rodd!* By now there were two weeks to go before the setting up of the science publishing company, and Piet, of course, was landed with all Willem’s travel and accommodation expenses.

At this point Ted called for help on Russell Cumley, a publisher colleague in Houston who had offered to finance him, and now had several manuscripts available.¹¹⁹ Already in 1951, and shortly after, Elsevier Press then managed to publish a hotchpotch consisting of perspective drawings, electro-cardiology and the microbiology of petroleum, all three by local authors. But it didn’t earn them a penny. Ted did have some success with a manuscript from an elderly lawyer, whom he happened to meet out there—again, by chance! This Edward Parsons from New Orleans had managed to collect almost all the documents on the historical library of Alexandria, and this fist-thick book that Ted published in 1952 found the requisite buyers.¹²⁰ However, it didn’t fit in with his vision of science publishing. Or did it? If we look at how he chased success, then perhaps he relinquished this more serious goal and recognised, instead, the frivolous and unscientific as being a condition for any undertaking, and their success as a source of *truth*. Here in Houston he had witnessed the culmination of an optimistic belief in progress and the message that life can be a happy adventure. Whatever was going on elsewhere, here it was all in the shrill tones of C major, and it was this public façade that spread America’s message of happiness with an unremitting radiant smile. This was the world of good health, a world where health was a marketable product... No wonder, then, when they first met in 1950, that Russell Cumley immediately told him all about his idea to publish a small medical encyclopaedia for the layman, together with the cancer specialist at the local hospital, Randolph Lee Clark.¹²¹ And no wonder either, that Ted, who knew all about publishing encyclopaedias and also had the means of cheap production, was immediately keen on the idea. So, even before the publications mentioned above were realised, he had already set out on a crash program; like everything that in America seemed to be a good idea, it was promptly carried out with breath-taking energy.

¹¹⁹ Klautz p. 197; *Chronicle* pp. 90–91.

¹²⁰ Klautz p. 200; *Chronicle* p. 92: ‘Parson’s *Alexandrian Library* was a brilliant work that was sold out in a couple of years, but too specialised to have it reprinted.’

¹²¹ Klautz pp. 197, 199, 204, 208–209.

Together with Clark and Cumley, Ted made plans for a *Book of Health*, with 860 pages, 72 tables and 1274 illustrations, 82 in colour, which would be put onto the market in 1951, the following year, for the modest price of 10 dollars.¹²² Clark, who was director of the hospital, got specialists to write articles (also from the renowned Mayo clinic at Rochester near Minneapolis), and Cumley re-wrote their articles in layman's language. Ted, himself, managed to galvanise printers Enschedé and binders Proost & Brandt in the Netherlands into a lightning production of 75,000 copies (although an expensive market research company in St. Louis predicted a million copies and, in time, fifteen million buyers), and to persuade the American Medical Association in Boston, who were not permitted to advertise, to recommend the work. 'We see that all notions by which ordinary people are accustomed to explain nature do not indicate the nature of anything, but only of the notions.'¹²³ *The Book of Health* was published, with some delay, in the autumn of 1951, in the midst of a chaotic period in which Ted realised he should be in New York, not Houston, for the promotion. Without consulting anyone on the matter, he rented a luxurious office on Park Avenue, gave radio interviews left, right and centre, got ecstatic press reviews, and allowed in a smooth-faced young man to advertise the product, complete with a poster of a broadly smiling family grouped around 'the best book ever,' which perhaps explains how he managed to offload the entire edition to the booksellers in one fell swoop.¹²⁴ That autumn *The Book of Health* was number 9 on the list of best selling books in America. But all this said very little. The list was of sales to booksellers, not to all those beaming families. What remained unsold could be returned to the publisher, which is exactly what happened, and at disconcerting speed. Two years later, it was found that only 14,000 of the 75,000 copies were actually sold.¹²⁵ In the same period (October 1953) it was also discovered that Elsevier Press had a debt of 140,000 dollars, to which should be added the value of the unsold stock of books, which we estimate at 150,000 dollars.

'Amsterdam' couldn't possibly fail to notice such goings-on. The board met to deliberate upon the non-success of their president-director. It was Dolf who conveyed to Ted the *truth*:

¹²² *Chronicle* p. 92.

¹²³ Curley pp. 445–446 [in the *Appendix* of Spinoza's *Ethica* I].

¹²⁴ Klautz pp. 211–213.

¹²⁵ *Chronicle* p. 110.

He told me that the directors had called a meeting and requested him to inform me that after my fiasco in Houston it would be advisable to stand down, with a substantial financial settlement and, of course, on condition that I should not compete in any way with Elsevier.¹²⁶

In March 1952 Ted offered his resignation. ‘Greatly regretted’ is how the commissioners recorded it in the minutes, for

...we admired the leadership given by the two directors. Mr Klautz and Mr Van den Brink are two quite different characters, but this is why they complemented one another so outstandingly. Klautz, who provided the broad outline with such exceptional imagination and energy...was supported by Van den Brink, who initially only managed the production side of the publishing but gradually took over leadership of the whole company.¹²⁷

Ted’s dismissal (honourable!) took effect on the first day of 1953, because there were various financial matters to be attended to. After that he went sailing.

Biochimica et Biophysica Acta

He had barely left, and reorganisation began. We have already mentioned the sale of the *Encyclopaedia of Organic Chemistry* to Springer-Verlag. In 1953, the sale of ailing, as yet unmentioned, Elsevier science journals entered the agenda. There were two: *Analytica Chimica Acta*, established in 1947 and edited by Phil West in Baton Rouge (Louisiana) and Alison MacDonald in Birmingham, and *Biochimica et Biophysica Acta*, established the year before and edited by Hendrik Westenbrink in Utrecht, with the assistance of Kaj Linderstrøm-Lang in Copenhagen and Claude Fromageot in Paris.¹²⁸ At Elsevier Willem Gaade was responsible for their publication, while Piet Bergmans took care of their loss (10,000 guilders per year). After the sale of the *Encyclopaedia*, when Yvonne Meijer-Praxmarer needed new editing work and was asked to ‘mother’ the journals, she was promptly informed by Bergmans that this involved

¹²⁶ Klautz p. 215.

¹²⁷ *Chronicle* p. 99.

¹²⁸ Slater pp. 19–25.

selling them off to Cleaver-Hume Press in London. Cleaver's legendary reply was: 'I don't want them, but I do want you.'¹²⁹

How did Elsevier come by these then unmarketable journals? The history of the earliest, *Biochimica et Biophysica Acta* (*BBA*), is illustrative, and will, therefore, suffice. Moreover, we are talking about the development of the most voluminous science journal in the world, in which we recognise the fruitful enterprise of Johannes Pieter Klautz—his, and his alone. We need to remind ourselves here that during the war he had brushed up on his knowledge of chemistry with the help of three scientists. We have no letters to this effect, nor any other document, but a man such as he would undoubtedly have offered to publish whatever his 'coaches' thought worthwhile, even if only through gratitude. And not long afterwards, one of the three, Hendrik Gerrit Koob Westenbrink, took him up on his offer.

Westenbrink was born in 1901 in Assen, studied chemistry in Groningen, and in 1926 took his doctoral degree under Frans Jaeger, on the structure of aqueous sulphate crystals that could be derived by X-ray diffraction.¹³⁰ These physical beginnings of later work on the structure of organic matter remind us of the six years older Herman Mark who, as we have seen, also began in X-ray diffraction and founded an important journal. But, unlike Mark, Westenbrink had nothing to fear from Jew-haters in the 1930s. At that time he was assistant to the nutrition expert Barend Jansen, professor of physiological chemistry in Amsterdam, and in his laboratory, which had not yet built up any great research tradition, he studied physiological and metabolic processes in enzymes. In 1940 he made his most important discovery.¹³¹ He found that animal thiamine (vitamin B₁) contained a pyrophosphate group, a group that is responsible for the carboxylation (fermentation) of thiamine that was discovered shortly before by Severo Ochoa. Ochoa had made his name—and justifiably appeared in Gaade's little book—and this is how Westenbrink also got a name, although it has to be said that

¹²⁹ Yvonne's experience fits to the story told in *Chronicle* p. 69: '[Joseph] Cleaver suspected a snake in the grass and remained of the opinion that Cleaver-Hume should not take over the publication of these journals [*BBA* and *Analytica Chimica Acta*]'. In May 1947 Cleaver-Hume Press had taken over the role of Elmer Bates (note 37) as Elsevier's representative in London.

¹³⁰ Slater pp. 8–14. [This chapter is reprinted from an obituary by Max Gruber in *Biochimica et Biophysica Acta* **97** (1965) i–v.]

¹³¹ H.G.K. Westenbrink & D.A. van Dorp, 'Causes of the "activation" of the carboxylase system by free ancurin', *Nature* **145** (1940) 465–467.

his has not really lasted in the biochemical literature. Already before his international breakthrough he had made friends with Kaj Linderstrøm-Lang, during a year at the Carlsberg Laboratory in Copenhagen (founded in its time to fathom the ancient practise, whereby water, barley and hops, together with a bit of yeast, are heated to conjure up a strong and sustaining drink).¹³² He was not the only one to approach Linderstrøm-Lang, a highly successful Danish scientist, with an amazingly deep insight into biochemical processes—and not only in the workings of ‘the brewery’—that enabled him to describe theoretically what should normally have been tested by physico-chemical experiment. But although this method came to be taken for granted in biochemistry, it never really got a chance in the widely read *Enzymologia*.

Once again we are reminded of Mark, who also never managed to have his work on polymers published in the journal of American chemists. *Enzymologia* was established in 1936 by an elderly biochemist, Carl Oppenheimer, who had given a certain individuality to biochemistry with his creditable surveys.¹³³ In this journal he wrote mainly about medical and physical research on the effects of enzymes, and it was published by Wilhelm Junk who, like Oppenheimer, was a Jew from Berlin who fled to the Netherlands, and believed himself to be safe in The Hague. When the Nazis invaded in 1940, Oppenheimer envisaged trouble once more and invited Westenbrink to join in the editing, only to hand it over to him entirely the following year, when the first measures against the Jews came into effect. Shortly after this Oppenheimer became sick and died. The following year, 1942, we see the 74-year-old Junk who, after the plundering of his shop in The Hague and the burning of all his books, killed himself and his wife, rather than be seized by the Nazis.¹³⁴ That same year Westenbrink got Linderstrøm-Lang to come and join him in the editing. But if, by doing so, he had hoped to give new impetus to *Enzymologia*, then he had misjudged the interests of Junk’s heirs, and the Nazi disaster that would overtake many loyal authors. While Klautz stood poised, as it were, to take over the publication, and between 1942 and 1945 only one volume could be published, these two came up with the idea for a

¹³² Fruton p. 48.

¹³³ Slater pp. 3–7.

¹³⁴ Isaac Harpaz, pp. 11–12 in ‘Frederic Simon Bodenheimer (1897–1959): idealist, scholar, scientist’, *Annual Review of Entomology* **29** (1984) 1–23.

more general journal, alongside the specialist *Enzymologia*. It must have been in the last year of the war that they

decided that Western Europe definitely needed a first class journal, devoted entirely to all biochemistry and biophysics, and [that they] therefore accepted the invitation from Elsevier to edit it.¹³⁵

But what were the circumstances of this invitation? In an account of *BBA*, described by a future editor, it was said that Westenbrink approached Gaade, that Klautz got wind of it and that he then formally invited him to set up 'his' journal. This is out of the question. Gaade only came into Elsevier in September 1946, while the first issue appeared in the following January. Nobody can build up an international editorial team of seven and an advisory board of twelve scientists in four months. Also the first published article, by Stig Veibel and Gregers Østrup from Copenhagen, had already been submitted in March 1946. Finally, this fails to appreciate the personal relationship between Westenbrink and Klautz during the war. These two would have agreed on the set-up and terms early in 1946. By then Westenbrink had obtained his professorship for which he had waited fifteen years (starting from the time of his appointment as an unpaid lecturer); it was in physiological chemistry at the medical faculty of Utrecht.¹³⁶ And Klautz had already seen the *Journal of Polymer Science* (or, in any case, the so successful *Polymer Bulletin*) that Dekker published for Mark. Westenbrink saw an opportunity to present himself internationally, and Klautz, in his turn, saw a golden opportunity—for if he didn't know it already, then Dekker would have explained to him that science journals can be financed through their subscriptions, and become a steady source of funds.

Westenbrink and Linderstrøm-Lang wished to attract authors from southern as well as northern Europe, and invited the already mentioned Frenchman, Fromageot, to join the editing team. They wrote their title in Latin (*Biochimica et Biophysica Acta*), to indicate that authors could publish in French, English or German, with summaries in all three languages. Even so, in the beginning the editors were unable to attract many authors. In 1947 they only filled 548 pages, and even then one third

¹³⁵ Carl Ferdinand Cori, 'In Memoriam', *Biochimica et Biophysica Acta* **97** (1965) xii.

¹³⁶ Slater p. 11: 'His influence on Dutch biochemistry was immense. [...] His aim was, as he himself once stated: I hope that my pupils will be at 36 years, or preferably at 26 years, where I was, comparatively speaking, at 46.'

of the articles originated from the Netherlands.¹³⁷ While the number of pages would not exceed a thousand per year, and the number of subscribers stayed at 500, this entailed investing extra money. At this time *BBA* posed no threat whatsoever to the American hegemony in this particular professional field, for it did not actually publish more than *Biochemistry*, and much less than the *Journal of Biological Chemistry*, the journals of chemists and biochemists in the United States. Nor can it be because *Westenbrink's Acta* (the nickname it acquired) was barely able to attract biophysicists. It must have lain in its newness, and in the mediocre quality and topicality of the published work, although it must be said that an article by Theodor Bücher on photochemical scission, which was intended for the *Biochemische Zeitschrift*, did attract quite some attention.¹³⁸ Westenbrink, who did the lion's share of the editing—thus the nickname—and was not strict enough in his selection and the length of the articles submitted, so as to be able to publish at least one volume (around 600 pages) per year, then decided on something new. In order to make the journal more attractive, he decided to open a section for short reports and provisional results. He did this in 1951.¹³⁹ And it worked, although not straightaway, but thanks to this section the journal grew. This was because in the 1950s there were so many new discoveries (now people speak of the biochemical revolution) that scientists often just wanted to briefly set out their priorities, and in a temporary form. Similar sections, and letter journals as well, would soon be sprouting up like mushrooms. However, Westenbrink's was one of the first, if not *the* first; the market was ripe, and after four years he was able to publish just as many short articles (of one or two pages) as long ones.

Anyway, it is unclear to what extent management at Elsevier was involved in directing *BBA* in 1951. Bergmans who, two years later, would give instructions to sell the journal, could perhaps have suggested to Westenbrink that a section for short communications might attract

¹³⁷ Slater pp. 27–29 and 64–65.

¹³⁸ *Biochimica et Biophysica Acta* **1** (1947) 21–34; Theodor Bücher had made name by the demonstration that the oxidation of the aldehyde to 3-phosphoglyceric acid is coupled to the conversion of adenosine diphosphate (ADP) to adenosine triphosphate (ATP). [Fruton p. 288]; soon he published again in *Biochimica et Biophysica Acta* **1** (1947) 292–314.

¹³⁹ Slater pp. 29–36: 'There is little doubt that the introduction of Preliminary Notes and Short Communications contributed to the large increase in the number of readers of *BBA*.'

extra authors, because it was he who instructed 'mother' Yvonne to wipe out the loss with advertisements, after an unsuccessful sale. These disheartening signs of insolvency would disappear after 1955, when the accounts finally showed a small profit. It may be unclear in what way Bergmans contributed to *BBA*, but in any case he was deeply involved in the organisation of this potential success.¹⁴⁰ We know from Frank, the central figure in our next chapter, that in 1955 Bergmans could not stop talking about all the different aspects of the science journal that he was publishing: four substantial volumes a year to more than a thousand addresses across the whole world. Bergmans and Frank lived close to one another, in the suburbs of Haarlem, and when Bergmans discovered this he had immediately picked up the telephone to ask Frank if he could come over and talk about the business they had in common.¹⁴¹ Bergmans' stories spurred Frank on to publish something similar, and that was to be *Nuclear Physics*. The two publishers became friends who promised to support one another in time of need, also if one of them became sick, and to respect one another's specialisms: Bergmans 'did' chemistry, Frank physics—but to this we shall return. Obviously, with the growth of *BBA*, reinforcement was needed in the secretariat and editorial office. This task fell to Jan Meijer and his wife Yvonne, for Willem Gaade had no time, and when Yvonne had a child of her own to mother and resigned, Jacques Remarque was appointed. That was in 1958. We shall let Remarque tell us about this in his own words a little further on.

But to begin with, *BBA*'s growth was a source of worry for the editors. It was inconvenient for Westenbrink personally, because at the time he was caught up in his own scientific interests at the university in Utrecht—in 1962 it would be his turn to be *rector magnificus* (vice-chancellor)—and two years later he died. Up until then the professor had done almost everything himself, but now he became overwhelmed by a flood of short articles requiring fast editing. In 1956, when *BBA* was ten years old, he summoned—there is no other word for it—Edward Charles (Bill)

¹⁴⁰ Slater pp. 39–40, 48.

¹⁴¹ Daan Frank writes (p. 4 in the section '1955–1960' of *Memoir B*—note 5 of the Preface): 'Some days after the Frankfurt book fair where we had made acquaintance, Piet Bergmans phoned me and said it was too odd that we lived close together and didn't know each other. [...] In the friendship that ensued from our mutual visits, we agreed that there was no point in becoming hostile competitors. We decided to compete in a friendly fashion, without written contract, like gentlemen, whereby North-Holland would not publish in the fields of Elsevier and vice versa.'

Slater to Utrecht and palmed him off with the ‘short section’.¹⁴² At the time Bill had only just delivered his inaugural lecture on cell physiology and intracellular enzymes. Born in 1917 in Melbourne, he had succeeded in obtaining a professorship in biochemistry in Amsterdam through his remarkable work (such as an idea on the source of energy for oxidation in mitochondria), although this didn’t stop Westenbrink from patronising him. ‘Son’ Bill would take over the general editorship of *BBA* on the death of ‘father’ Hendrik. The two of them energetically set about to enlarge the editing team, with biochemists in the vicinity (Laurens van Deenen and Max Gruber—and later Piet Borst), and succeeded in contracting other well-known names for the journal from further afield (Alexander Braunstein, Erwin Chargaff, Carl Cori, Fujio Egami, Roger Flavell, Hans Krebs, Albert Neuberger and, later, still more).¹⁴³ Such solid grounding was needed, because specialisms were on the rise. People spoke of molecular biology, and of a journal with that name, which Academic Press began in 1959 and for which it had attracted outstanding authors. Would this pose a threat to *BBA*, or an opportunity?

Bill Slater and Jacques Remarque would exchange a few peppery words on the subject! We were still able to talk to Jacques about his contribution to the success of *BBA*. He was the sort of man who comes straight over if you want to know something about his work, together with a briefcase full of documents.¹⁴⁴

I was 32, and had just got my PhD in Biochemistry *Over fracties van leverglycogeen* (*On Fractions of Liver Glycogens*) and my tutor, Westenbrink, already had a job for me. That’s how things were in those days. Elsevier would employ me to organise *BBA*, and, of course, I went. When I arrived in 1958, *BBA* was an ailing journal, with hardly enough articles even for a quarterly, and when I left 26 years later, it was a weekly, the greatest journal in the world, not only in biochemistry, but of them all. People liked to joke about it: I was to have worked with *Elsevier’s Weekly*, you know the one, the news magazine published by Bonaventura, part of the Elsevier holding. But first I helped Jan Meijer sort out the *BBA* administration, because it was in such a mess, and step-by-step I learned the art of editing and publishing. It all seems so obvious, but even so, it can’t be repeated often enough: a journal stands or falls with the reputation of its editors—which is not quite the same as their reputation as scientists. Only

¹⁴² Slater pp. 44–45.

¹⁴³ Slater pp. 47, 50, 53.

¹⁴⁴ Interviews of Jacques Remarque on 1 and 28 May 2003.

then does speed of publication count. At first I was unable to change anything in *BBA*, but for other important journals where I was involved, like *Brain Research*, I approached the editors myself. What I could do was to work on publication speed. I allowed six months between submitting an article and publication (subject to approval, but I got that in two thirds of cases), and to realise this in an ever-increasing number of articles we needed to build up a good-sized editorial secretariat (under the supervision of Jan van Geelen and John Morris). To keep up with growth and maintain speed, I took on four printers: Meijer in Wormerveer, Dijkstra in Groningen, and Thieme and McDonald both in Nijmegen. Then I did a bit of pioneering with air freight so as to deliver on time to subscribers in America. But my main contribution to the growth of *BBA* was the battle with the general editor. We didn't agree. He fought for unity in biochemistry and was against fragmentation of the journal, and I fought for the interests of the publisher and wanted sub-division into sections, which could then be further sub-divided.¹⁴⁵ I had great respect for the Slater who worked on his cytochrome enzymes at the physical chemistry laboratory on the Jonas Daniël Meyerplein, but not for the undiplomatic Australian redneck, who could fly into the most terrible rages if things did not go as he wished. You could see him turning red in the face. I could handle it, though, and it didn't affect me.¹⁴⁶ In fact, I was the only one at Elsevier who could. And I knew how to get my way. The first section that I separated was *Nucleic Acids*, known later as *Gene Structure*. At that time, in 1962, the study of nucleic acids was rapidly becoming an independent discipline. The same applied to four other sub-disciplines, which later got their own section. We called it 'twigging'. You had to know just when to do it. Not too soon, because then you had too few subscriptions, and lost money. But not too late, either, because then other publishers had already captured the market. It was a real art.¹⁴⁷

We see the figures of *BBA*'s growth recorded in Jacques' documents.¹⁴⁸ In 1955 the journal had 1250 subscribers, five years later 2100 and

¹⁴⁵ Remarque described the twigging policy in two Elsevier-documents: *Problems posed by the growth of biochemical literature, in particular that of BBA* of 1 November 1965, and *Thoughts on the future of BBA* of 26 January 1970; this policy and the necessary adaptation of the editorial office are summarized in Slater pp. 58–59 and 100–101. Remarque liked to compare the growth of *BBA*, and the associated sectionalization of scientific disciplines, with an economic phenomenon (the *Matthew effect*) whereby rich nations become richer and poor nations poorer [pp. 1624–1628 in *The Bookseller* of 5 September 1970].

¹⁴⁶ Slater pp. 46 (on the division in sections) and 71–72 (on the twigging-clash of 1967).

¹⁴⁷ See note 144 of this chapter.

¹⁴⁸ Most data are from Remarque's *Aide-Mémoire BBA* of 22 August 1963 that starts with the conclusion '*BBA* has become the commercial cornerstone of Elsevier's Science Publishing Company'; other data are from Slater pp. 78–79.

again, five years after that 2650; but in these years the number of pages was, respectively, 1800, 5400, and 14000—an almost logarithmic rise. In pages, it already surpassed its greatest competitor the *Journal of Biological Chemistry*; this occurred, to be precise, in 1962. We see the same strong growth in the financial figures. In 1955, expenses were at 0.09 million guilders, five years later 0.35, and five years after that 1.15, while subscriptions brought in, respectively, 0.13, 1.10 and 2.90 million guilders. So over a period of ten years the profit increased from the paltry 0.04 to the fair-sized 0.75 to the substantial 1.75 million guilders. This growth would continue after 1965. In 1970 the journal would reach 16500 pages, five years later 21100 and again, five years after that 26200, while subscriptions in these years would generate 4.2, 8.4 and 13.3 million guilders. We have estimated these latter figures from the number of pages, the average price per page per subscription, and the number of subscriptions that, after a peak of almost 2800 in 1966, gradually decreased to 2100 in 1980. And in fact, the librarians started to complain of the exorbitant costs for all those metres of *BBA*. Jacques didn't disclose the profits in the 1970s, but they must have been gigantic.

The man who went sailing lies off a faraway coast. He has sailed out into the morning haze, enshrouded in salty mist. The 'Bonaventura' has barely left the harbour, the yacht plunges into the breaking waves of the ocean's shifting boundary. She thrusts her bows into valleys of grey water, her propeller momentarily grinding the air, and is heeled over with Archimedean force, while green water cascades across her deck; then, more calmly, she recovers her flowing cohesion in bubbles of spume as they stream behind in her foaming wake. Add to this the noise—a thunderous pounding, a groaning in which the ear distinguishes a rumble, a creaking, grinding, even a boom. And add again to this the light—sparingly piercing the vapour, refracting in ruddy trails of foam the dull grey fountains over the prow, where the eye may spy a veritable archway of colour, although the sun still lies behind the high coast. To awake once and for all.

When he has passed through the surf, the skipper hoists the main-sail, then the jib. His ship steadies in the northern breeze that fills the flapping sails and drives them over to port.

CHAPTER FIVE

FRANK AND HIS 'NORTH-HOLLAND'

This chapter, too, begins in the darkness of Nazi Germany. The autumn of 1934 was extremely disturbing; at the time Daan Frank was looking around in the Akademische Verlagsgesellschaft in Leipzig, as he was later to write. This 21-year-old young man from the Netherlands was on a work placement. He was already determined to become a publisher and hoped to learn a great deal at the Akademische. However, as the son of a Jewish father he knew very well that it was important that he looked like a typical German from the north, for this had been explained to him by the 'race theoreticians' in Leipzig.¹

He felt threatened, a feeling that first came over him on discovering the revolver that another student on work placement carried with him, not openly but secretly. The young man explained that every member of the SA (*Sturmabteilung*) possessed such a weapon.² On top of this came the news that all SA leaders had been shot on Hitler's orders, just like that—without the presence of a judge, and without any trace of evidence that the SA was about to seize power. It was Daan who had to tell them at the Akademische what was being said in the Dutch press—nobody trusted their own press any more—and they couldn't stop talking about it.³ But not a word of this in his letters, not even hints. People didn't trust confidentiality of the mail either. And when he did need to record something of the menace that prevailed, he did it indirectly: 'The German soul is filled with national socialism, which, as Goebbels says, cannot be exported. But I hope to see the day when, once again, Germans will write books that can be understood abroad, and appreciated.'⁴ He was thinking of Thomas Mann, while in Neurenberg

¹ *Memoir K* p. 9.

² *Memoir K* p. 10.

³ *Memoir K* p. 14.

⁴ *Memoir K* p. 13; 'Ich glaube Göbbels hat gesagt daß das deutsche Nazional Socialismus kein Exportartikel sei. Kann man denn fordern daß N.S. Bücher exportiert werden? Meine Herrschaften, ich muß kurz sein! Der deutsche Geist ist jetzt noch ganz erfüllt vom neuen Leben, aber der Tag wird kommen, so hoffe ich, daß die deutsche Schriftsteller wieder Bücher schreiben die man im Ausland verstehen und schätzen kann.'

the Nazis were gathering for their monstrous party rally, filmed as *Triumpf des Willens*...

In the circumstances we can easily understand why Kurt Jacoby, the director who had made the Akademische so renowned, seized this opportunity to sound out Daan, young as he was, on the prospects for scientific publishing in the Netherlands. The trainee gave the director the address of his father, who knew publishers in his own city of Haarlem, and the two did, indeed, speak to one another in October.⁵ Jacoby must have already had plans to move his publishing business over to the Netherlands, or England or the United States, but as we know from Chapter 3, he never succeeded. However, in that autumn of 1934, Jacoby must have spoken urgently to this trainee about the menace of anti-Semitism in Nazi Germany, otherwise we can't explain the feeling of solidarity that brought Daan back years later to plead Jacoby's case. 'On 13 December 1938 I received a telephone call that Jacoby had been arrested and was in prison. That same evening I took the night train [from Amsterdam] to Leipzig, and the following morning at 10 o'clock I was at the prison. I pleaded on his behalf and declared that I was prepared to pay for an exit visa. Whether or not this contributed to his being released a few days later I can't verify. It was not the last time, however, that Jacoby got picked up by the police.'⁶

Father and son Jolowicz, Leo and Walter, hardly saw Daan as they worked mainly in the antiquarian bookshop Gustav Fock, also owned by the Jolowicz family. It was housed elsewhere and separate from the publishing business. Who he did see regularly, besides Kurt Jacoby, was the gregarious executive secretary and Gina, the secretary who was prone to popping in each morning and throwing up her skirt with the cry: '*Und Herr Frank, was finden Sie heute von meinen Hüften?*' ('And, Mr Frank, how do like my hips today?') This was before the arrival of the two young scientists, Erich Proskauer and Julius Podolanski, with

⁵ *Memoir K* p. 18. In a preceding paragraph he reflected on Jacoby's fortune and misfortune: 'Jacoby depended entirely on the Jolowicz's. The large, luxuriously furnished house in Leipzig where he lived with Agnes Jolowicz, his wife, was the property of his father-in-law. After a youth in Insterburg in East Prussia and a study of art history in Munich, he had worked for Teubner and Springer. There he had learned how to attract and bind important scientists as authors. When he started to work for his father-in-law, it was his success as a science publisher that laid the foundation of AKA's fortune. There is no doubt about that. Later, when he had fled to America, he again was dependent, now on his brother-in-law, who had the family capital and used it to found Academic Press.'

⁶ *Memoir K* p. 19.

whom he shared a room, and who were in charge of science publications. Proskauer, the more dextrous of the two, was born in 1903 in Frankfurt. He studied chemistry in Leipzig in 1925 and came to work at the Akademische while still a student.⁷ In 1931 he had the chance to earn his PhD, and in 1937 to escape to the free world—we met him already in the previous chapter. Podolanski was born in 1905 in Tarnov, Poland, and came to Germany at a young age.⁸ He had studied physics and thanks to his remarkable dissertation on a subject in wave mechanics, also in 1931, was appointed by Werner Heisenberg as his assistant. Nothing would have stood in the way of a university career if he had not been Jewish, but with the Nazis in power this was out of the question.⁹ In 1933 he lost his citizen's rights and his job, and had been without work for several months before Proskauer, also a Jew, took his

⁷ *Memoir K* p. 17; 'Erich Simon Proskauer' is also mentioned in *Biographisches Handbuch der Deutschsprachlichen Emigration nach 1933* (Saur-Verlag, München 1980), where one finds that he married in 1931 (the year of his doctorate) to the dentist Jenny Diment, and that he then got a research position at the Institute for physical- and electro-chemistry in Leipzig until he had to be fired, in 1933, because he was a Jew.

⁸ *Memoir K* p. 17.

⁹ From a letter of 14 November 2004 of Marietje N'Jie-van Rossem to the author (Marietje was married to Julius Podolanski from 1948 until his death in 1955): 'Julius was born in 1905 in Tarnov (Poland), but soon afterwards his parents emigrated to Germany and lived a while in Jena, then in Sondershausen, where four further children were born. In 1924, after the gymnasium, he went to Leipzig for a study of physics. In 1931 he got a doctorate *magna cum laude* for the dissertation *Die Anwendung der Ritzschen Methode auf Polarisationsprobleme in der Wellenmechanik*, elaborated under supervision of Georg Joos—see also *Annalen der Physik* V **10** nr 6. In 1932 Werner Heisenberg gave him a temporary position as assistant. Although the Jewish Podolanski family had got the German citizenship in 1920, it lost it again when the Nazi's seized power. From 1933 on Julius was stateless, and initially also without work. After a year or so he found work as corrector at the Akademische Verlagsgesellschaft. In this job he discovered real errors, not printing errors, in a paper on the foundations of quantum mechanics by Hendrik Kramers, written for the *Handbuch und Jahrbuch der chemischen Physik*. In a letter of 13 November 1937 Kramers wrote: *Sie haben manche wissenschaftliche Fehler entdeckt, an den ein anderer der nicht—wie Sie—die moderne Quantenmechanik versteht, völlig vorbeigegangen wäre*. The year before, he had sent a draft paper of his own to Kramers, Erwin Schrödinger and Max Born, to which the first two had reacted with helpful comments and the third with a blunt rejection. Since the draft refers to the 1921-paper by Theodor Kaluza on a unified world picture, it could have been a precursor of his 'Unified field theory in six dimensions', that was worked out during the war and was in published in the *Proceedings of the Royal Society* **201** (1950). [Note of the author: because it was a purely formal exercise in Riemannian metric, it didn't have much to do with physical reality.] In the years after the war he would also work on the theory of fluctuation phenomena, on the origin of the elements, and on astrophysical problems (*Philosophical Magazine* VII **45** (1954) 13). Meanwhile Kramers had done his best to find money for Julius' appointment as assistant, so that he could leave Germany. In August 1939 he arrived in Leiden.'

case to heart and employed him to check formulae and revise printers' proofs. It would take fifteen years before Podolanski could prove his true talent with a unification of electromagnetic and gravitation forces in a six dimensional space. Daan, who also did little else than monitoring and checking—for him this involved mainly quotes and invoices in the journal administration—was witness to Podolanski's excitement when, besides misprints, he discovered fundamental errors in the printers' proof of *Grundlagen der Quantumtheorie*. No wonder he was excited, for the text belonged to Hendrik Kramers, a celebrity. Kramers, alerted to his errors, hastened to thank this unusually well-qualified proof reader, and in 1938, when Podolanski sought refuge, managed to secure him a post as assistant in Leiden. We shall meet Podolanski again later.

So who was Daan Frank?¹⁰ Menkes Daniël, as Daan was really called, was born in 1913 in Haarlem as the son of a paediatrician. Small of stature and weak of constitution—he struggled long with diabetes—he was long-sufferance itself, but possessed a will of iron and immense self-discipline. Growing up amidst towering shelves of medical literature that also contained the latest French and German novels, he developed a voracious appetite for literature from a young age. He read everything regardless of whether or not he understood it, and in the confusion that must have followed an *idée fixe* was born. One day, when his father found it necessary to remonstrate with him about a bad school report, he declared that the gymnasium wasn't right for him: it would not get him where he wanted. Daan, this 15-year-old, wished to become a publisher of books. Father Emanuel, confronted with such decisiveness, took him out of the gymnasium and enrolled him at a commercial college, with the firm intention of financing his education in publishing as if it were the academic training that he had originally had in mind. Daan's passage through college was smooth—just imagine if he had made a mess of it! He then worked as an apprentice in a bookshop in Amsterdam, and afterwards with a publisher in Haarlem. And he just kept on reading. At the periphery of the business, he learned the value of social activity, of concerts and dance halls where he could make friends, and how he could use his personality to good advantage. Then came the 'university' of the foreign work placements.¹¹ He spent one year in Leipzig, first at a commercial college, and then at the Aka-

¹⁰ *Autobiography*.

¹¹ *Memoir K* pp. 6–28.

demische, where we began this chapter. After this he spent a second year in Paris, at the booksellers and publishers Gallimard, and then finally a short period at a small publisher in London. He cut short this last work placement when he was offered a job at Noord-Hollandsche Uitgevers Maatschappij in Amsterdam. That was in 1936.

The Noord-Hollandsche had a staff of three. This small company was set up in 1931 by Gerrit de Vlugt, the owner and director of a Calvinist daily, *De Standaard*, and of a printing works called Holland, where the newspaper was printed.¹² In this way, he could satisfy the request of the Royal Dutch Academy of Sciences, for whom he already printed their Proceedings and books, to also take care of the delivery, sales and administration of its publications. As the Academy paid for all expenses this limited liability company needed no capital. However, he deposited a sum of 150 guilders so as to have at least some funds in hand. He held the shares and the Academy, with its three professors, had control over the board. Although it went under the grand name of Noord-Hollandsche Uitgevers Maatschappij and was allowed to call itself the Academy publisher, this small company was little more than an operation for subsidised printing work. Its office was in a room on the Nieuwezijds Voorburgwal in Amsterdam, directly beneath large attics that were packed with offprints, and high above the basement where a thunderous rotation press spewed out the newspaper.¹³ De Vlugt rarely appeared, spending most of his time with the newspaper; and there was little turnover. In the fifth year, 1936, when Daan entered

¹² *Memoir K* p. 30: 'The publishing company was founded in Amsterdam on 13 August 1931, in the presence of notary G. Ruys, while G.C. de Vlugt and W. Verheggen were attending. The latter was managing clerk of the printing company Holland, a function he was also going to fulfil in the new company. The following men were nominated in the Board: W. de Vlugt (Mayor of Amsterdam and father of G.C. de Vlugt), P.A. Diepenhorst (professor at the Free University) and three members of the Academy, C.U. Ariëns Kappers, J.J. Salverda de Grave and L.E.J. Brouwer. The director of both the newspaper *De Standaard* and the printing company Holland, G.C. de Vlugt, became director of the new company as well. He got two shares, and Verheggen one, but these three were immediately handed to Holland, so that the Noord-Hollandsche Uitgevers Maatschappij became a daughter of Holland and a grand-daughter of *De Standaard*.'

¹³ *Memoir K* p. 33: 'The publishing company had one room on the first floor of the N.Z. Voorburgwal 68–70. It measured 15 square metres, and being at the backside of the building it looked out in the Spuistraat. I sat there together with a young gentleman D. van den Heuvel, who had worked at Elsevier's, and a typist. There still was a package-boy, but I couldn't count on him since he also was De Vlugt's private driver. Above us were two floors, each of 150 square metres, stacked with *Proceedings*, all unsold publications since 1808, and lots of brochures of a few pages that still were for sale for 80 cents.'

the company, twenty-four titles were published, of which only two, on economic growth and Assyrian documents, were of any significant size.¹⁴ Such passiveness was strange for Daan with the working practices of the Akademische in Leipzig still fresh in his memory. He heard about plans to publish some book series, four volumes on a geological expedition to the East Indies and twelve volumes on a description of Dutch law, to mention just a couple, but these were not concrete. And besides, what would be the market value of such works? So, without waiting for the end of his first year at Noord-Hollandsche, Daan had already written a memorandum in which he advocated the publication of German or English language works, together with specialisation in the exact sciences.¹⁵ For, as he had seen in Leipzig, this was where demand lay and where money was to be made.

The memorandum from 1937 must have been written by Daan Frank as an assignment to himself, because not only did he begin to carry out this plan, but he stuck to it as well. So it is a pity that this document no longer exists: the entire Noord-Hollandsche archive perished due to water damage in the 1970s. When he submitted it—to the director because he needed money, and to the governors because he needed authors in the exact sciences whom the Academy would have to supply—his chances of success seemed small. De Vlugt said that there was no money available and that he would just have to earn the necessary capital himself. The Academy was more helpful. They introduced him to Jan Tinbergen, the economist who attracted considerable attention with his *Statistical Testing of Business-Cycle Theories* and who now wanted to publish, together with several professors of literature, a series of popular books *Uit leven en wetenschap* (*From Life and Science*).¹⁶ They would not be in German or English, nor would they be on the exact sciences, but could entail a substantial print run. Daan's director gave him a free hand and he decided to publish the series, only to discover after

¹⁴ 'Fondscatalogus 1949' p. 12 [F.M.Th. Böhl, *Assyrian charters*] and p. 15 [I.J. Brugmans, *Economic cycle in the nineteenth century*].

¹⁵ *Memoir K* p. 39: 'In the spring of 1937 De Vlugt asked me to write a memorandum for the board [with its members from the Academy] about my ideas to transform North-Holland in a publishing company for the exact sciences, with books in German or English.'

¹⁶ *Memoir K* pp. 38–40; p. 71 of the 'Fondscatalogus 1949' specifies the authors of the series of booklets *From Life and Science*: A.L. Hagedoorn, A.M. Meerloo, J. Tinbergen, G. van der Leeuw, H.J. Jordan, A.C.J. de Vrankrijker, N.B. Tenhaeff, G. Révész, H. Oldewelt, F. Sassen, J.J. Fahrenfort, B.A. van Groningen, D. Loenen, B. Delfgaauw, and J.M. Kramer.

a while that there was little profit to be had from it. However, he also received a tip from the Academy to have a word with a civil servant at the Ministry of Economic Affairs, who was writing textbooks on business administration for a retailer's diploma. As these textbooks were to be made compulsory by the Ministry, there was bound to be plenty of demand for them.¹⁷ Daan saw his chance and straightaway signed a contract to publish them, without consulting his director. De Vlucht was abroad, and unavailable. In hindsight, it was a golden tip, and he would sell almost a million of them. But he would have to wait for ten years before the sale of all the books produced a small capital. A war would intervene—the war that Nazi Germany would bring upon her neighbour, the Netherlands—and trade would be frustrated by plunder and destruction. In the years 1938 and 1939, when the required capital was nowhere on the horizon and the threat of war from the east brought uncertainty to all those around him, Daan was already searching for authors who could help him in his 'exact' plans.

He did this with patience and logic. Without any great knowledge of the exact sciences, to which this man of letters had never, anyway, felt particularly attracted, he already recognised that chemistry offered greater scope, or at any rate had more applications, than physics. We are speaking of the 1930s. Whoever was looking for business had to be in chemistry. In Amsterdam alone, there were already two publications in this field, the *Chemisch Weekblad* and the *Pharmaceutisch Weekblad*. In the same city Johan Nordemann's bookshop, which was actually run by Maurits Dekker, himself a chemist, had for years been doing good business with the import and export of chemical literature, as we wrote in the previous chapter. Everybody knew in Amsterdam that, on Dekker's instigation, Klautz of Elsevier had bought the rights to *Karrer* and *Richter-Anschütz*. And if this still didn't sufficiently prove the value of chemical science, then Klautz' initiative in publishing an *Encyclopaedia of Organic Chemistry* finally convinced Daan Frank. As a beginner he had no access to these men—and he would never have the opportunity to

¹⁷ The 'Fondscatalogus 1949' specifies on pp. 47–48 the dozen very successful booklets on accountancy (the so-called *Middenstandsserie*) by Frederik Leendert van Muiswinkel, about which Frank writes (*Memoir K* p. 38): 'The success gave me prestige. De Vlucht showed me his respect, and also the men of Holland who had to make reprint after reprint. It also led to the allotment of paper during the war years so that we could survive as a publishing company.'

speak to Klautz—but he understood that it would be better to keep away from chemistry.

Choice for Physics

He would have to make do with physics, a less crowded field, but also a field in which Martinus Nijhoff in The Hague ‘already published everything of significance’.¹⁸ Here we see Daan, nevertheless, eagerly on the hunt for authors. They should be accessible and, of course, have a name. Hendrik Kramers? To approach such an important figure he would need an introduction and Sybren de Groot, a young physicist who he happened to meet and who still had to write his dissertation, would be unable to arrange it. In the end it was chance that came to his aid. As we already know, in 1938 Daan was in Leipzig trying to arrange a release for Jacoby, and it was here that he heard that Julius Podolanski was about to leave for the Netherlands to become Kramers’ assistant. It was already June before Julius was properly settled in Leiden, and when Daan looked him up that summer in the Kamerlingh Onnes Laboratorium, Kramers was away. But he did find Hendrik Casimir, recently appointed as professor to study the causes of superfluidity. This didn’t stop him from establishing relations with Gillis Holst, director of the Philips Physics Laboratory in Eindhoven and also professor *extra-ordinarius* in Leiden. When Daan told Casimir that he wanted to publish books on physics in English, he was promptly referred to Holst. The head of one of the largest industrial laboratories in the world would certainly have something to publish. Daan:

In the first week of May 1940 I was to have an interview with the director of the Physics Laboratory in Eindhoven. As agreed, I arrived at 11 o’clock in the morning and was shown into the waiting room. Fifteen minutes passed, half an hour. Then the secretary came to say that the director had been unexpectedly detained and that I’d better go home. It was only when I got there that I heard about the war alarm of that morning. They’d had other things to think about at Philips than a publisher’s plans!¹⁹

¹⁸ Cited from a taped interview of Daan Frank by Han Kruyswijk and Pieter Bolman on 20 November 1985; this interview also gives details about Frank’s visits to physicists in Leiden, which are lacking in *Memoir K*.

¹⁹ *Memoir K* p. 40.

The German invasion on 10 May put a stop to these plans, and for the time being Daan couldn't expect any English book manuscripts from the Philips Laboratory. But the war had something else in store for Daan. In the precariousness characteristic of war, Daan came into contact almost immediately with the man who turned out to possess the key to the great success that he would later enjoy as a science publisher. And once again it came about indirectly through Podolanski.²⁰ It so happened that one of the first measures taken by the Germans was to order the departure of foreigners from the coastal region. So Julius, barely a year in Leiden, was forced to leave and went to Utrecht. As a stateless Jew the university would be closed to him, but even so, it seemed to him to be the best place to work on the 'unification' in six dimensions, his life project. He found accommodation in a windmill, at some distance from the city. Here he met up with other students, including Daan's sister, Truus, and it was through her that Daan heard about Podolanski. Beneath Julius' conscientiousness, already well-known from his time at the Akademische, there lurked a courtly clown, someone who took an ironic distance from himself and was always prepared to help others. He heard, as well, about Julius' visits to the secret lectures on theoretical physics that a newly appointed professor held at his home. In the beginning of 1941, the helpful clown, who at that time already wore the Jewish star on his coat, introduced Daan to this professor. Later that year, when Julius had refused to register himself with the Germans for deportation to the east and went underground, Daan was

²⁰ From a letter of 14 November 2004 of Marietje N'Jie-van Rossem to the author (sequel to note 9): 'In May 1940 Julius had to leave Leiden. For a year he lived in a mill in Vreeland that was owned by Arie Bijl, a colleague physicist he had met in Kramers' group. By the way, that mill burned down in 1943. I met him there at Whitsuntide 1941 and fell in love with him. [...] From 19 September 1941 on he had to wear a yellow star. The few times I visited him in Utrecht he had one on his coat. Then, in August, he got a call to report himself in Amsterdam—it was not stated why, but he knew it was for transport to Poland. When he got a second call he hid himself in various, constantly changing addresses in Leiden and Oegstgeest; in the summer of 1942 he may have lived for a couple of months again in Arie's mill. Arie, who took care of his shelter, was caught in April 1944 while he was hiding English pilots, was sent to a German concentration camp and did not survive the war. Julius' health had suffered from the stress and the lack of exercise and fresh air. After the liberation he got sciatica, was treated for three months in the Academic Hospital in Leiden and had to rest for many more months at home. Only in 1946 he was sufficiently recovered that he could move to Utrecht, where Léon Rosenfeld wanted him as assistant.'

given this man's inaugural address to print: *Development of the Idea of Causality*. His name: Léon Rosenfeld.²¹

Léon Rosenfeld, born in 1904 in Charleroi (Belgium), was multi-talented. As a schoolboy, on one reading alone, he was apparently able to learn by heart and recite large pieces of text—a whole page of Goethe's poetry, for example. His mythical memory meant he quickly felt at home in many different languages—he even wrote letters in Latin without difficulty—and buried himself at a young age in *Parmenides*. But he possessed a quick understanding in other fields as well. Barely 22 years old, he obtained a doctorate in mathematics and physics, with the greatest possible honours, at Liège and, after just four years, he became professor there after learning quantum theory from pioneers such as Max Born in Göttingen and Wolfgang Pauli in Zurich. In that year, 1930, his idea on gravity-quanta—a first attempt to relate quantum mechanics with the general theory of relativity—made such an impression on Niels Bohr that he wanted him as his assistant in Copenhagen. From then on Léon would continually be on the road, and not only from Liège to Copenhagen and back. He loved travelling. He was the man, for example, who in the beginning of 1939, after arriving in New York with Bohr, straightaway took the train to Princeton to tell Einstein the sensational news of the discovery in Berlin of nuclear fission. In 1933, together with Bohr, he delivered proof that quantum mechanics could be harmoniously related to classical electrodynamics—that is to say, retaining the correct, experimentally tested predictions of classical electrodynamics. And, just before the war, together with another assistant of Bohr, Christian Møller, he began work on an explanation of the strong force with which protons and neutrons attract one another, a force that is generated by the exchange of other particles with alternating symmetry. After 1940, the two of them would exchange long letters in which they worked out this idea together, and after 1945 Léon would examine fully this so-called meson-exchange in *Nuclear Forces*, the first English book that was published by Noord-Hollandsche.

However, we are getting ahead of our story. There is something else we must point out. The man who came to Utrecht in 1940 to teach theoretical physics was, contrary to what the above-mentioned

²¹ G.E. Brown, 'Léon Rosenfeld 14 August 1904–23 March 1974', *Nuclear Physics A223* (1974) i–viii; to this commemoration and assessment of the scientist M.D. Frank added: 'Léon Rosenfeld—Author, Editor, Friend', *Nuclear Physics A223* (1974) ix–xi.

work may suggest, more a philosopher than a physicist. His inaugural lecture on causality already shows clearly that he was first and foremost preoccupied with the question of which kind of uncertainty quantum theory—the most important discovery in physics of the twentieth century—actually introduced into the description of nature.²² Although he said that he only clarified Bohr's ideas on the subject, in fact it was he who gave us the 'Copenhagen interpretation' of quantum mechanics, an interpretation that had to revert to former notions on determinism, and therefore took on an historical dimension. Early on he had already begun his editing of Bohr's work, not as an assistant or a secretary, but in the role of Socrates who brought out what was best in his discussion partner.²³ Inevitably he went on to reflect upon the irreversibility of natural processes—a statistical causality?—and about something as miraculous as the development of the mind. 'The theory of knowledge plays a greater role now than in classical times; in order to fulfil this role it needs still to be developed according to strict scientific method, a task in which logicians, physicists and psychologists must work together.'²⁴ This is what the solemn, but good-humoured Léon said to Daan, his publisher, in 1972. We don't know what they spoke of in 1941. They became good friends, but in his old age Daan was unable to remember much about their early discussions. Understandably, for even if he did his utmost to harness a man of such great erudition, he also had other matters on his mind. Wasn't it about time that he, a half-Jew, went underground?

The war, which brought an abrupt end to Daan's plans, also threatened to bring to an end his work at Noord-Hollandsche, and to every kind of security. Immediately after their occupation of the Netherlands, the Germans demanded an Aryan declaration from all civil servants so as to find out who among them was Jewish. All the Jews were promptly dismissed. It was unclear, however, whether Daan was to be dismissed as well. He was not fully Jewish, nor was he fully a civil servant. The publishing company did seem to be a branch of the official organisation of the Academy of Sciences, but on the other hand belonged to

²² Léon Rosenfeld, *Ontwikkeling van de causaliteitsidee*, Noord-Hollandsche Uitgevers Maatschappij, Amsterdam (1942); *L'évolution de l'idée de causalité*, M. Hayes—Imprimeur de l'Académie royale de Belgique, Bruxelles (1942).

²³ Bohr (*Collected Works*) 1–3, the volumes edited by Léon Rosenfeld.

²⁴ Quote from Rosenfeld's speech at the retirement of Frank, in: 'Contemplations of the future at a farewell' [*Toekomstbespiegelingen bij een afscheid*], unpublished brochure (1972).

a newspaper. Moreover, the director, Gerrit de Vlugt, had managed to have the Noord-Hollandsche shares put into his own name, just before the Germans put a *deutschfreundliche* manager in control of the newspaper in 1940. So that meant Daan was employed by De Vlugt personally. To divert attention from him even further, he was relieved of his title of assistant director that he had been given the year before.²⁵ He signed no more documents and disappeared into anonymity. So this is how we see him, this 'North German Aryan', in the fourth winter of the war, as a servant who helps the administrator of the Academy onto the tram to the suburbs, where the director gives a jubilee luncheon.²⁶ He writes very little about this period. Only this:

A role in the Resistance was not for me. They needed me at the office, and at home, too, in the family, where I was the only man. [Father Emanuel had died suddenly in 1937, and the Jewish grandparents Frank were deported in 1942 never, we understand, to return.] I went to the office by train and by bike, later on foot, at least if there was no threat of razzias for young men. If there was any danger, I stayed at home, close to the hideout. For the rest, I brought round food vouchers, visited people in hiding and accompanied them to new addresses.²⁷

As it turned out, by some diabolical logic, Daan's family required his full attention just when he was least needed at the office, where all had gone quiet. During that summer, just as the inhabitants of Amsterdam were witnessing the first large-scale deportations of the Jews, the German government had rudely informed the Academy, via an order to the publishing department, that articles written by Jews were no longer to be printed and that articles in English would not be accepted. The latter affected, in particular, the Proceedings, which had been published in English since 1898.²⁸ The Academy submitted to the German orders under protest, since the alternative of suspending all activities would be going too far. So publication of the Proceedings was stopped, and not only these. Due to this intervention by the occupier, in the autumn of 1942 the usual stream of publications by Noord-Hollandsche was

²⁵ *Memoir K* p. 41.

²⁶ On 15 February 1944, a bookkeeper of the Academy used Daan Frank as guide to find De Vlugt's home outside Amsterdam, so that he could hand *azalea mollies* (a 50 guilder present from the Academy) for the 12.5-year jubilee of North-Holland, to supplement a written congratulation of the day before from the Secretary of the Academy, Martinus Woerdeman.

²⁷ *Autobiography*.

²⁸ Lusenet, Yola de, in: Schoonhoven p. vii.

more than halved.²⁹ After the survey of natural science research in the Netherlands, which was the first written material on the subject of any coherence, nothing very astounding appeared: just books with hundreds of pages on Dutch pronunciation, or else on the literary life of the sixteenth century in one or other of the provinces.

A picture of dull fatalism—unable to rebel, taking pride only in its past. Academic science, this summation of certainty and probability, which is unable to predict the uncertain passage of history, produces only images of perfection. After the failed attempt of the English to cross the Rhine at Arnhem and rid the country of the Germans in one fell swoop, the fifth wartime winter came as a great blow. Daan wrote: 'Plenty of ideals, but no transport, cold, hardly anything to eat.'³⁰ The densely populated region lying below sea level in the west of the Netherlands was, and is, the most vulnerable part in the whole of Europe in time of modern warfare, and by threatening to submerge it the occupier exploited this vulnerability to the bitter end. Starvation for four million people. Only at the end of April did permission come from Seyss-Inquart, the *Reichskommissar* appointed by Hitler, who had orders to defend the beleaguered West to the last, to allow food to be supplied by air. Hundreds and hundreds of low-flying Royal Air Force bombers dropped 6 million kilos of food parcels with flour, yeast, sugar, milk powder, pulses, bacon, sausage, chewing gum, chocolate and... cigarettes.³¹ Just days after the capitulation on the 5th of May there was another bloody shooting in Amsterdam. Of the following summer, Daan wrote only that he rented a room on the Herengracht and met a girl, Anna van Westrienen, to whom he quickly became engaged, and four months later they married.

We don't know whether the atom bomb played a role in the renewed contact between Daan and Rosenfeld, but almost certainly it must have done, because Daan's first publication after the war was Rosenfeld's very relevant *Essay dedicated to Niels Bohr on the occasion of his sixtieth birthday*. It is thanks to this publication that the name North-Holland came into the world. 'The name Noord-Hollandsche for our English publications is just unthinkable,' as Daan said forty years later. 'We'll

²⁹ According to the 'Fondscatalogus 1949', 24, 31 and 23 titles were published in the last three years before the war, in the six years from 1940 until 1946 one had 36, 36, 26, 12, 11 and 13 titles, and in the first three years after the war 39, 42 and 55.

³⁰ *Autobiography*.

³¹ Jong **10B**, pp. 1297–1298.

make it into North-Holland. And that caused such a commotion in the Academy that I changed it into North-Holland without their permission...³² When we remember that the publishing company was an independent firm, we can hardly imagine what objection there could be to such a change of name. Unfortunately very little record is to be found in the Academy archives of this 'commotion', which must have been in late 1945, only a letter dating from January 1946 in which we read that there was 'no overruling objection' to the English name, a choice of words that indicated that objections did exist.³³ But which? Surely not against the language of the liberators, the true *lingua franca*, the only one eligible after the war for Daan's plans to publish works in the exact sciences? It had to do with a symbol, with the relativisation of the national identity.

The liberation provided Daan with yet another opportunity. Not only freedom of language, but also a free hand. Gerrit de Vlucht, who had been removed for some time from the newspaper and from Holland, the accompanying printing business, but who couldn't be held responsible for the *deutschfreundliche* past of the newspaper and was absolved of all blame, went to England to look for work for his printing business and returned with a large number of orders.³⁴ These had to go through first, so printing assignments for North-Holland, who were obliged to use the Holland printers, had to wait. This led to tensions with the Academy who also suddenly wanted to publish everything at once: in 1946 no less than 39 titles, more than anything before the war. Relations became more and more strained, until the ill-fated moment arrived when Daan had such a row with the manager of Holland printers that he threw a whole pile of books at him. Upon being summoned by the director, Daan summarily told him that he would leave and look for another job, if he wasn't allowed to fulfil his responsibilities as a publisher. De Vlucht was soft-hearted. He swallowed his anger and offered Daan a number of his shares in North-Holland, thereby giving him an equal say in the

³² Cited from a taped interview of Daan Frank by Han Kruyswijk and Pieter Bolman on 20 November 1985.

³³ Letter from M.W. Woerdeman to Frank of 30 January 1946; Frank had written him a few days before that an English name of the company ('albeit not a very fine one' but 'already in use for some time') is a necessity, since the Dutch name 'cannot be kept in mind in an Anglo-Saxon country' and 'has given already difficulties in the exploitation.'

³⁴ *Fragments* p. 43.

publishing company.³⁵ And so it came to pass. And because De Vlugt was involved only with the newspaper and the printing business, this meant in practice that Daan had obtained a free hand.

Monographs on Theoretical and Applied Physics

Meanwhile Daan went once more to Philips, where Casimir was now director, to discuss his plans. When the Germans closed Leiden University in 1942, he had moved to Eindhoven where he succeeded Holst at the Physics Laboratory,³⁶ and was able to write up a survey of the symmetry relations in thermodynamics before his new position demanded all his attention. After the war it quickly became clear that in America knowledge had grown enormously in those physical and chemical processes that provided the basis for Philips products worldwide. Casimir needed to engage with these developments; also he felt that in many respects the Netherlands quickly needed to catch up on lost ground. He was, therefore, certainly keen to help North-Holland become a world publisher in the physical sciences, but because Philips published its own *Natuurkundige Bibliotheek* he was obliged to maintain a certain distance.³⁷ However, he did agree to become an honorary editor to the company, and persuaded Hendrik Brinkman to do the same. Brinkman, an experimental physicist who had worked at Philips, was appointed director of the Arnhem test laboratory of the electricity companies in 1944, and in 1950 would move to Groningen to revive experimental research there (with a particle accelerator to generate and study nuclear reactions). His insights and contacts would make him invaluable to North-Holland, and in 1948 when he was first appointed, together with Casimir, as editor of Daan's *Monographs on Theoretical and Applied Physics*, he turned out to possess innumerable contacts abroad.

³⁵ *Fragments* p. 46: 'I bought 2/5 of the shares of the company for a price that was fixed by accountants, and wished a clause in the new regulations that he [G.C. de Vlugt] and I had to be unanimous in decisions on appointments, profit etc., so that he could never overrule me, although he held 3/5 of the shares. And since De Vlugt didn't pretend to know anything about publishing, he agreed with everything I proposed. Our relation could be called friendly, but in the early fifties we had a conflict, mainly about the salary he got as co-director. It was as high as mine and he got it gratuitously. We needed the mediation of a commissioner [J.D.J. (Dick) Roos] to solve the conflict.'

³⁶ Casimir pp. 260–282; The immense influence Casimir has had in this position is indirectly described in the *Festschrift* that was edited by Sarlemijn and Sparnaay.

³⁷ *Fragments* p. 48.

It is also generally accepted that he was strongly involved in the award of the Nobel Prize in 1953 to Zernike, the man from Groningen, for phase contrast microscopy.³⁸

In 1949 Daan managed to find a third member for the editorial board under his own steam, in Amsterdam, where he worked. We have mentioned that he had got to know Sybren de Groot, who did a PhD on thermal diffusion immediately after the war and then worked as an assistant at the Van der Waals Laboratory. However, when Daan approached him for his advice he had just returned from a work placement at the French *Commissariat à l'Énergie Atomique*, and had other things on his mind.³⁹ He referred Daan to Jan de Boer, his predecessor at the Van der Waals Laboratory, who had meanwhile become professor of theoretical physics in Amsterdam. Although Jan de Boer had his own specialism, namely in statistical mechanics, he was thoroughly convinced that new insights into physics could only be obtained if study of the subject remained broad-based. So he did not hesitate to join Daan's team. The broad-based and at the same time specialist series *Studies in Statistical Mechanics*, which he later edited together with George Uhlenbeck, also helped to establish North-Holland as a science publisher.⁴⁰ In 1960 Hendrik van Bueren, who did his PhD on the description of lattice defects in crystals under Casimir and then became professor of the astrophysics laboratory in Utrecht, joined as the fourth member. Van Bueren repeatedly insisted that North-Holland should provide the handbooks and journals that were needed for impoverished Dutch physics, once crowned with Nobel Prizes for Lorentz, Zeeman, Van der Waals and Kamerlingh Onnes, to be restored to its former glory.⁴¹ All

³⁸ Berkel, Helden & Palm pp. 609–611: Frits Zernike was a skilled instrument-maker, who in a study of optical images had found that there is a phase difference in the diffraction of light by different substances. When he had also found ways to increase this phase difference, he was able to build a microscope for the study of living cells (until then the microscopy of cells required staining, which usually killed them). In 1936 he had already obtained a patent for this invention. Henk Brinkman was convinced that it deserved a Nobel Prize as well and sent his arguments to the Nobel Committee. For Frank the convictions of Brinkman were very useful, so that he called him 'the most original' of his advisers (*Fragments* p. 48).

³⁹ Peter Mazur, 'Sybren Ruurds de Groot, 8 april 1916–9 mei 1994', *Nederlands Tijdschrift voor Natuurkunde* **60** (1994) 173–174.

⁴⁰ Boer & Uhlenbeck *passim*.

⁴¹ Interview of H.G. (Henk) van Bueren on 18 February 2003: 'After the war Philips, Shell, AKU and DSM—Dutch industries with large international markets—played a crucial role in the renewal of scientific research at the Dutch universities, which between 1945 and 1950 was almost non-existent. They granted advanced instru-

four were strongly international in their outlook, often knew potential authors personally, and were willing to offer suggestions. The spirit of the times was idealistic. They were content to receive the books from the publisher's list, as well as with the dinner parties after their meetings, at which Daan showed himself to be a lively and erudite host of refined taste.

The honorary board was not yet in office when Daan's first handbook came out, and obviously this was to be Léon Rosenfeld's great work on nuclear forces. After his meticulous handling of Léon's oration on causality and his Bohr essay, it now fell to Daan to create a monumental work out of the ever-growing and formulae-strewn *Nuclear Forces* manuscript, or at least to produce a substantial book with substantial pretensions.⁴² For isn't the Parmenides quote with which it opens

χρεω δε σε παντα πυθεσθαι, ημεν Αληθειης...

rather pretentious? "now you shall hear of all things, of the Truth...." It is a pity that the theory elaborated in it is only half the truth. Due to the time the author took to process the data that had been obtained during the war in America, and his move to Manchester (where he obtained a more suitable chair at Rutherford's renowned laboratory for nuclear research), the book could only be published in 1948.⁴³ When it finally did come out the most important information then available was missing. As it happened, in 1948 the elementary shell structure of the atom was discovered, which could in no way be explained by meson exchange alone, and would mean a Nobel Prize for Maria Goeppert-

ments and gave advice. Only in the 1960s the industry withdrew so to say from the university. In the 1950s I was working at Philips and studied there a.o. imperfections in crystals. Up-to-date textbooks were scarce at the time—in fact one only had those of McGraw-Hill, which were excellent though—and therefore I wrote one on my subject [see Bueren] and offered it to Frank, when his North-Holland publishing company still was very small. Daan Frank was glad with my manuscript and asked me to join his group of advisers. Here I supported Jan de Boer in his wish to develop a list of textbooks written by Dutch physicists.' [Note: Van Bueren, who had started his career as an astronomer, left the field of materials research in 1965, however, and founded a laboratory for the study of astrophysical processes that were largely irrelevant for the North-Holland list.]

⁴² Rosenfeld passim.

⁴³ *Fragments* p. 48: 'Already in 1947 Rosenfeld accepted a chair in theoretical physics in Manchester, and Podolanski went with him. It delayed the publication of *Nuclear Forces* but it gave me the opportunity to visit Manchester from time to time. In May I used to bring them a small kite of herring, and thereby a lot of fun. On one of those pleasant evenings I met Gerry Brown, the later editor of *Nuclear Physics*.'

Mayer and Hans Jensen. Surprisingly, demand for the book hardly suffered as a result and the entire edition of 4000 copies was sold, but a reprint of the speculative and clearly incomplete treatment by Léon was not worthwhile.

The book turned out to be a learning process not only for the scientist, but also for Daan. Holland printers struggled with the complicated typesetting, and Daan had to learn how to galvanise printers not only into achieving perfection, but also speed. 'This book is about secrets of the atom bomb,' he told them, melodramatically, 'and lives depend upon speedy publication.'⁴⁴ Often he had to travel to Manchester, where twice, in the month of May, he presented his author and assistants with a keg of herrings, thereby earning the name of 'enlightened capitalist'. He even thought he had to galvanise these gentlemen into action, as well. They included the young and colourful American, Gerry Brown, who would later become an important journal editor for him, and... Julius Podolanski, who became Léon's assistant after the war and had joined him in Manchester. Julius devoted all his time and energy into correcting the proofs and adding final touches to the text, wrote Léon in the foreword, followed by: 'It is impossible to say to what extent each page has been improved by his typographical experience and critical reading of the text. Not infrequently have I been cheered when things were going against us by his inexhaustible patience and good humour.' Daan remembered later Julius's sarcastic letters to Amsterdam about even the smallest printing error.⁴⁵ This Julius Podolanski, who had led such an unhealthy life, under constant stress and with long periods in hiding, died only a few years later of a heart attack. For all that, the

⁴⁴ Cited from a taped interview of Daan Frank by Han Kruyswijk and Pieter Bolman on 20 November 1985. The section 'De druk van het drukken' (*The pressure of printing*) of *Memoir B* specifies the problems Frank had to get his books printed in De Vlucht's printing company: 'Immediately after the war De Vlucht had acquired many printing orders from publishers in England, since Holland was relatively cheap. However, to the horror of father and son Boomsma, his type-setters, he had promised unrealistic terms of delivery, which meant that the printing orders of North-Holland were often put aside. [At that time North-Holland was contractually bound to have its books printed at Holland.] Every week I went to De Vlucht to complain about the delays, every week I got vague promises, and every week we spoke about potential charges by MacMillan and his other English publishers, or my authors, that contracts were breached. Besides that, Holland didn't have monotype, but only linotype, which was old-fashioned and time-consuming, being unfit for the type-setting of complicated formulas.'

⁴⁵ *Memoir B* p. 3 in the section 'Edward Guggenheim'.

most important lesson Daan had to learn was how to get his books onto the American market.

Without lists of potential buyers and groups of buyers in the United States, it was impossible to sell anything from Amsterdam. So he needed a representative in America for North-Holland, and in the autumn of 1946, long before *Nuclear Forces* was completed, Daan flew to New York to negotiate with acquaintances who had sought refuge there about the sale of his book.⁴⁶ He called on Kurt Jacoby who, with Walter Jolowicz, had set up Academic Press—the American version of Akademische Verlagsgesellschaft. He also visited Jacoby's former editor in Leipzig, Erich Proskauer. Together with Maurits Dekker from Amsterdam, Proskauer had set up a science publishing company with the fine name of Interscience. We met these four names already in the preceding chapter. In comparison to Jacoby and Jolowicz, who had managed to bring over part of the Gustav Fock antiquarian book stock, in the beginning Proskauer and Dekker had found life hard and they both had to supplement their income with cleaning jobs and washing up. Fortunately, during the war their publishing business grew. Was it the story of Podolanski's trials and tribulations and his role in the realisation of *Nuclear Forces* that Proskauer was so resolute in putting himself forward? In any case, Interscience seemed to offer the best conditions. Its business director, Dekker, wished to purchase as many as 2000 copies, subject to a discount of 60% and on condition that they would be unbound, in the form of loose sheets. This meant he could stamp Interscience on the back of the bound copies, which he promptly did two years later.⁴⁷ Daan had not foreseen such a move and gnashed his teeth: his first great work, the pride of North-Holland, in America bore the name of Interscience. It taught him to leave nothing to chance when it came to contracts.

He learnt, too, never again to spoil a good deal. Rashly he let himself be fobbed off with the Dutch rights to a book by Bausch & Lomb on the human eye. He saw that it was highly specialised, but was beguiled by the splendid transparencies of eye cross-sections. It flopped, and so badly that he removed the title from his catalogue and wished never to see it again. But it had been at his own risk and didn't spoil his relations with Interscience. Despite frictions, Proskauer and Dekker remained his

⁴⁶ *Fragments* pp. 52–57.

⁴⁷ *Fragments* p. 55.

main anchors in America. ‘They gave me information that I’d find difficult to obtain elsewhere,’ Daan wrote later, ‘on dispatch lists, discount policy, typesetting methods, binding cardboard, and other technical matters, also on author’s contracts. That’s why, after a while, I could make my own way in the American market, at least in the disciplines of logic and econometrics in which I obtained detailed dispatch lists. But in the wider field of physics I still depended on them.’⁴⁸

Meanwhile, with the publication of *Nuclear Forces*, Daan knew how he wanted his second handbook to be: less thick and less pretentious than the first, and for a wider market. Casimir frowned when he heard what the subject was because it seemed to have little new to offer, but Sybren de Groot had seen the text and told Daan that it was worthwhile.⁴⁹ The title was *Thermodynamics—An Advanced Treatment for Chemists and Physicists*. The author, Edward Guggenheim, derived thermodynamics from a different set of concepts than had been used before. For the first concept, like everyone else, he chose temperature, something concrete that can be experienced as heat, but for the second he chose something abstract that is not to be experienced, entropy (a measure of the probability of molecular order in a chemical system), and not energy as was more conventional though not experienced any more directly. De Groot found the choice for Guggenheim attractive because thermodynamics as it had been developed until then held good only for systems that are entirely in equilibrium. However, in reality this is never achieved. It was already known to the ancient Greeks that there are always currents, however slow they may be: *πάντα ρεῖ*. The time was ripe to extend the scope of thermodynamics, in which the assumption of a constant value of entropy (which is only correct in equilibrium) is replaced by the realistic assumption that it changes, or can change. And this is what De Groot was working on. So it made sense not only for Guggenheim, but also for De Groot to take entropy as the point of departure, precisely because of its changeability, rather than energy, which is a system quantity, that is to say it is unchangeably the same. We see again this point of departure in his *Thermodynamics of*

⁴⁸ *Fragments* p. 56.

⁴⁹ Casimir p. 398: ‘Even in Guggenheim’s textbook, which on the whole is very reliable, I found formulas with errors, not just in the first printing but also in the second.’

Irreversible Processes, a pioneering work that Daan published as his third handbook in 1951.⁵⁰

With an introduction from Sybren, Daan went to see Edward. His account of their meeting in 1948 is so splendid, and so characteristic of the relationship between publisher and scientist at that time, that we give a generous quote:

At the station exit [Reading, England] a short little man stood waiting, hatless, spotty-faced and tousled white hair, a copy of De Groot's dissertation tucked under his arm by way of identity. We drove to his laboratory in a smart new car, quite at odds with his dishevelled appearance. When we got there he pulled on a dirty white overall over his suit, blending once more with his surroundings, which were extremely dingy-looking. We then embarked on one of the most difficult conversations with an author that I have ever had, and it seemed as if I was inadequate on all counts: in expressing myself in English, in my experience of international copyright, in knowledge of his subject, but above all in the ability to negotiate with extremely difficult people. Guggenheim wanted a reprint of a book that had been published by Methuen in 1933 and which he had completely re-written. The royalty he demanded was so extravagant I hadn't a clue what to say. Was this the reason he wanted to have me, an inexperienced small publisher from Holland, now that he had quarrelled with Methuen? He behaved correctly, but was bad-tempered. After about an hour and a half, during which time I became thoroughly exasperated, I said that I would think it over, that I would now like to return to London and would write to him. "Oh, no!" he said, "We're going to have lunch at my house." I said that I had an appointment in London. He pushed the telephone towards me. "Do please telephone them in London and cancel your appointment." I didn't have an appointment in London at all, but was thoroughly fed up with the whole conversation and wanted to get back to my hotel. But the fellow was so insistent that I phoned the porter at the hotel and said that Mr X, who was to visit me at 3 o'clock, was to be given the message to come back again in three hours time. We drove into town, out again, went through endless suburbs, then up a hill until we stopped in front of a splendid country house, complete with tennis court and swimming pool. A woman came out to meet us, very attractive, Simone Guggenheim, French. Edward left me alone with her, disappeared upstairs then came down again ten minutes later, beautifully dressed. We began with sherry, then were served a delicious lunch, while we continued our conversation in French. . . . I don't know anymore just how I managed to set up the contract, but Gug got his way in everything, also in the

⁵⁰ Groot *passim*; the Preface (p. vii) tells right away that 'the introduction of non-equilibrium thermodynamical functions gave rise to the setting up of an entropy balance equation in which the notions occur of entropy flow and entropy production.'

full royalty of the selling price of copies for the American market that Interscience bought off me with a 40% deposit. When he saw from the proofs that they were set in linotype, and not in monotype, he blew his top at me. But I was committed to Holland printers, where monotype wasn't yet in use. However cleverly they were able to supplement linotype with hand type and compose the most complicated formulae, the result still looked old fashioned and not as refined as monotype. Podolanski had already made some unpleasant jokes about it, but Gug's comments were exceptionally biting. He wouldn't tolerate typesetting errors, even in the proofs.⁵¹

Guggenheim's *Thermodynamics* was a hit. There were five reprints, and over 15,000 copies were sold. Casimir may have had his reserves, but when the book finally came out he couldn't deny that it was sound, exceptionally sound even, but with the comment that the first printings still contained a few errors. This success showed in the accounts of the publisher as well as of the author, and not just in the literal sense. Because the author wished to pursue those matters that had earned his caustic comments during publication—expressed, incidentally in beautifully written letters—Daan found himself making frequent excursions to Reading.⁵² There he spent long evenings drinking port and smoking cigars, playing silly games with 21 questions, then spending the night, one day to be informed that the Frenchwoman had taken her life. Clearly he had allowed himself to be overawed by this improbably rich, yet grubby little professor. After he had published two more of his books—the first with success but the second without—their relations cooled. 'In the 1960s Gug fell for an offer from Maxwell, who was probably the only man who could impress him, and edited an encyclopaedia of chemistry for Pergamon, which flopped.' Let it be stated here, however, that Guggenheim's *Thermodynamics* has become a strong consolidated basis for the tower of material science, which is not in itself in any way elitist, but reputable and practical, at the service of trade and industry.

It was quite a different story with Sybren de Groot's book. But then how different was their relationship! In 1945, Daan printed his dissertation, *L'effèt Soret*, and the business relationship between the young scientist and his publisher, just one year older, quickly turned into friendship. Daan recognised something of himself in this conversationalist with his

⁵¹ *Memoir B* pp. 1–2 in the section 'Edward Guggenheim'.

⁵² *Memoir B* p. 3 in the section 'Edward Guggenheim'.

subtle jokes and infectious laugh, or perhaps he saw his own reflection in Sybren's nervous enthusiasm and love of reading, especially French literature. How could such a brilliant physicist be found in such a man of literature? He reminded Daan of Léon Rosenfeld; but Léon had left the Netherlands too quickly for any close friendship to develop. An evening spent with Sybren meant an evening spent discussing Malraux or Teilhard de Chardin, whose philosophy he criticized in a legendary article in *Hollands Maandblad* and, if Daan felt so inspired, about encyclopaedia projects, the old one of Diderot and d'Alembert, and the new one of Neurath, a member of the *Wiener Kreis*, who sought to propound the relationship—indeed, the unity—of science and literature.⁵³ Every evening spent with Sybren fed Daan's ambition to contribute his own cultural project, and we shall see where this ambition finally led him. Naturally their friendship meant that Sybren had all his books published by Daan. There would be five.

Here we shall discuss only Sybren's *Thermodynamics of Irreversible Processes*. This was his second work after Daan published his dissertation, and appeared in 1951 as part three of the already-mentioned *Mono-graphs on Theoretical and Applied Physics*. It was a pioneering work, differing therefore from that of Guggenheim, and tied in with a recent study by Ilya Prigogine. With beautiful mathematical formalism, De Groot brings unity in the description of diverse types of currents that may occur at the same time, then interact with one another and in this way contribute to their irreversibility—which leads to extra entropy.⁵⁴ But in spite of Jan de Boer's enthusiasm for the work, Daan did not print very many at first, because it struck him as being too obscure. To his surprise, though, the initial small demand continued, and eventually the book was reprinted six times. In the meantime Sybren continued to

⁵³ Interview of Anneke Frank-van Westrienen on 4 November 2004; Sybren de Groot also published in *Libertinage* and *Tirade* (literary periodicals like *Hollands Maandblad*) about André Malraux, Simone de Beauvoir and Italo Svevo [see note 39 of this chapter]; the frequent visits of Daan Frank and his wife to Sybren de Groot and his wife were in Amsterdam, where they always kept a weekend-home—also during the years they had to live in Utrecht and Leiden because of Sybren's professorships in these towns (information of 8 March 2005 by Leendert Suttorp, co-author of the textbook *Foundations of Electrodynamics* [North-Holland, 1972]).

⁵⁴ The beauty of this treatise added much to its impact on the scientific development of the author. In the interview of 29 October 2002 Nico van Kampen called it exceptional: 'Books can, but rarely do, influence the development of a new field; famous examples are the books of Sommerfeld, Dirac and Von Neumann, and not to forget the one of De Groot and Mazur.'

work on the subject in Utrecht where he had succeeded Léon Rosenfeld. Here, with the assistance of Peter Mazur, a text twice as thick was published in 1962 under the title *Nonequilibrium Thermodynamics*. A pioneering work had become a standard work and up until 1980 would be cited as many as 1350 times.

Quest for Journal Editors

These three works gave North-Holland international renown, but financial success would still have to wait. In 1950 the company even had problems with its liquidity, and was running at a loss. 'For a few days things were really critical,' Daan said in 1985, 'and I had no idea how on earth how I'd make it till the next year.'⁵⁵ He and Gerrit de Vlugt both put almost ten thousand guilders into the company, in the form of a loan, which could only be paid off years later. And why was this necessary? Not only did the sale of Guggenheim's *Thermodynamics* take a while to get going, there was also a gap between the agreed payment from the Academy for their expenses and associated rising costs.⁵⁶ There were no reserves, because in the first years after the war profits were too meagre to be able to put money aside. For this they would have to wait until after 1955. In that year for the first time North-Holland profits must have been more than the 5%, on an estimated turnover of half a million guilders, which is considered desirable for healthy operational management.⁵⁷ Daan describes this reversal in fortunes: 'In 1956 we doubled the profits and turnover increased by one third, of which three quarters was in export—we'd become international!'⁵⁸ Unfortunately we have no figures (the archives having been lost), but in Daan's papers we read that in 1962, in more prosperous times, he

⁵⁵ Cited from a taped interview of Daan Frank by Han Kruyswijk and Pieter Bolman on 20 November 1985. *Fragments* p. 94: 'To solve liquidity problems in 1950 both De Vlugt and I lent North-Holland 8,425 guilder, so 16,850 guilder in total; this loan was paid off in 1954.'

⁵⁶ *Fragments* p. 47.

⁵⁷ This guess is based on the following lines in *Memoir B*: 'The booklets of [Frederik] Van Muiswinkel [note 17] gave a profit of 20,000 guilder, and the one by [Simon] Korteweg and [Frans] Keesing (*Het moderne geldwezen, Modern Finance*) 11,000 guilder. The normally profitable *Almanacs* gave a loss, though, since in that year (1955) the sales had stayed behind the costs of editing and mailing. [Thanks to 'Muiswinkel' and 'Korteweg & Keesing'] our net profit became 38,000 guilder.'

⁵⁸ *Fragments* p. 49.

was able to take over De Vlugt's remaining shares after a favourable re-capitalisation of the company.⁵⁹ By then De Vlugt was well into his seventies, still enjoying a generous salary for duties he never had to carry out, and already blessed with a newspaper and printing works—which he sold shortly afterwards, anyway.

It was not only financial worries that plagued Daan in the 1950s. The three ladies and one gentleman who took care of editing and bookkeeping at the North-Holland office on the Amsterdam Nieuwezijds Voorburgwal may have been hard-working, but they couldn't help him with acquisition, marketing or the finances. 'For a man alone, what I did was actually impossible, and then to have to mark up typesetting instructions for the formulae with green, red and blue pencils until deep into the night...'⁶⁰ He still lacked the specialists for his specialised publishing company. Also he had too little opportunity to travel for the acquisition and supervision of the manuscripts, something he regretted, since talking to authors was the icing on the cake... So as soon as he was able, which was not until 1956, he took on for the acquisition a physicist, Wim Wimmers, who could approach those authors recommended to him by the editing team. Then in 1962, when he must have had between ten and twenty staff members, he took on Engelbart (Bart) van Tongeren, an experienced publisher, to assist him in marketing and finance. The latter has a chapter all to himself, Chapter 6, so great was his influence on the publisher's list. But here we shall limit ourselves to the development of the publisher's list in physics, which Daan had begun to build up.

Now we look especially at the North-Holland journals. Once again, there are three titles that draw our attention: *Nuclear Physics*, *Nuclear Instruments* and the *Journal of Nuclear Materials*. The company's growth in the 1950s is largely thanks to these three titles, especially *Nuclear Physics*. Daan may have known that monographs contained little that was new (for only well-researched material can be written clearly), but even so he had not expected that the three monographs that he had published would sell so badly. How could this be, when physics was enjoying such explosive development? Brinkman and De Boer, with whom he discussed the matter in the summer of 1952, thought it was not specialisation

⁵⁹ *Fragments* p. 94.

⁶⁰ *Memoir B* p. 1 in the section 'Van Tongeren, Wimmers, Krips & Baltzer'.

that restricted his market, but speculation—or rather, the lack of it.⁶¹ Whoever wanted to find out something new read science journals, not books. Only in journals did one find speculative, often only half understood results of research that stimulated the mind and aroused critical comment. The American *Physical Review*, which they also read extensively, was becoming thicker and thicker and this was because of the increase of contributions on nuclear physics. In Brinkman's opinion, therefore, there could in future be a demand for journals specialising in nuclear physics, and this might mean an opportunity for North-Holland. But if this was the case, how should Daan take advantage of it? Piet Bergmans, a colleague publisher who often came to see Daan—he lived close by—and often talked about *Biochimica et Biophysica Acta*, kept insisting that a large, specialist journal could only be successful if its editing team was not just competent, but international as well. And because most of the news on nuclear physics came from America, he would have to find an American general editor, or else one with good connections in America. So in the first week of September 1952 Daan attended an international conference on beta and gamma radioactivity that was held in Amsterdam, where Hendrik Brinkman introduced him to Kai Siegbahn.⁶²

Kai Siegbahn was Swedish, but also half American. At the conference in Amsterdam he had proposed that the force with which electrons are bound in an atomic nucleus can be accurately determined with special spectrometers—a proposition that would later earn him a Nobel Prize. On meeting Daan, the friendly and communicative Kai promised him a book on nuclear spectroscopy, for it should be easy to produce such a book with so many specialists there at the conference and others whom he knew from his travels. However, Daan did not immediately invite him to edit a journal on nuclear physics. Kai was then only 34 years old. We had the privilege of speaking with this exceptional man in Uppsala when he was 85, still full of memories of this first meeting with Daan, as well as many others, and eager to help record the history of science journals 'because they are so important for the growth of knowledge.' He tells us about his role:

⁶¹ Interview of Daan Frank by Han Kruyswijk and Pieter Bolman on 15 November 1985.

⁶² *Memoir B* p. 1 in the section 'Kai Siegbahn'.

My ideas and discoveries cannot be separated from editing the work of my colleagues. To edit a text means to make it understandable, to clarify it and put it in order. It is an exciting confrontation with what is new. To understand why I always found it so important and spent much time at it, I need to say something about my career. I was born in 1918, took my doctoral degree in Stockholm in 1944 and visited America with my father straight after the war. My father was the Nobel Prize winner Manne Siegbahn. He was visiting his old friends Lawrence, Oppenheimer, Fermi, Rabi and others, to hear what they had been doing during the war, which he pretty well knew, anyway. So I got to know these great men at a young age. In 1946 I returned to America alone, this time for a year. By now physicists were entering the universities again, after having completed their military service in the laboratories specially set up by the army, such as in Los Alamos where the bomb was made, and they brought with them what no-one had heard about. I travelled a lot and saw something new in every university laboratory. My aim was to learn to measure the speed of electrons from radioactive nuclei (the energy in beta-radiation), but *en passant* I saw experiments to bring nuclear spins in a strong magnetic field in resonance with high frequency radio waves (NMR). That was really exciting. Edward Purcell was working on it on the East Coast and Felix Bloch on the West Coast, and because I was flying to and fro I was able to tell the one what the other was doing. They listened with interest, and we know, don't we, that both were awarded a Nobel Prize for it? This confrontation with great experimenters taught me that once back in Sweden I would only have a chance of finding out something new if I had sophisticated instruments. Men like Bohr and Fermi knew the value of experimental innovation, but they were exceptions. It was already quite something for articles on experiments to devote just a couple of lines about method and instruments, usually straightaway in the introduction, but so brief as to be of no real use. That's why I was keen to edit *Nuclear Instruments* for North-Holland when Daan asked me to. Actually he only asked me to do so in 1956, when *Beta- and Gamma-Ray Spectroscopy* had been published, the book on nuclear spectroscopy that I had promised him. To come back to the usefulness of direct confrontation, at times I felt quite confounded by the detailed correspondence I had with the 42 authors. While I was still often able to speak to them at conferences, hardly anyone actually kept to the subject that I had given them, which is understandable when new things were being discovered all the time. 'The rose of Göttingen', Maria Goeppert-Mayer, gave us the explanation for the shell structure of the nucleus. No one wanted theoretical speculations any more, like those of Rosenfeld. And what other names did I have? I had them all... This tremendous collaborative project cost me three years, but we could all be satisfied with the result.⁶³

⁶³ Interview of Kai Siegbahn on 14 November 2003.

Not only this; it would be an indispensable work, altogether a thousand pages, with 4000 copies printed.⁶⁴ Ten years later, still with the same worldwide demand and twice as thick, it was revised and re-published, this time in two volumes. Then, in 1965, there were 2500 copies printed, with 77 authors needed to write up all the new knowledge. What caused the greatest sensation? The recoilless gamma-emission discovered by Mössbauer, or the violation of parity conservation in beta-decay? (In nuclear physics parity is a quantum number that can only be +1 or -1.)

And what about Kai Siegbahn's own work, and his relationship with Daan Frank? On publication of 'their' book on nuclear spectroscopy, Kai had just become head of the Physics Laboratory in Uppsala. He organised a college on nuclear spectroscopy for the dozens of PhD students who descended upon him, and a laboratory where he supervised their work to increase the resolution of electron spectrometers—in fact these were instruments from the Nobel Institute in Stockholm, where he had obtained a professorship after his stay in America. Once the resolution was truly high, strange things came to light: the force by which the electrons in the nucleus were bound turned out to be influenced slightly, but unmistakably, by the environment of the nucleus. This was totally unexpected. To understand the environmental influence, the 'chemical shift' was then systematically measured, for almost all elements of the periodic system. Incidentally there was no radioactivity emitted with these measurements, because the electrons did not come from a beta-decay, but were beamed in. In the course of this work, Kai developed a highly sensitive chemical analysis technique (ESCA) for which he was awarded a Nobel Prize in 1981.⁶⁵

We wonder how Daan managed to keep such an authority as Kai Siegbahn as author and editor all to himself. In the 1950s there were plenty of publishers who threw themselves into the field of nuclear

⁶⁴ Siegbahn *passim*.

⁶⁵ *Memoir B* p. 3 in the section 'Kai Siegbahn': 'I heard the news of Siegbahn's Nobel Prize on the radio, and immediately sent him a telegram with congratulations. It appeared to be the first foreign telegram he received.' In the interview of 14 November 2003 Siegbahn told that his discovery of the chemical shift was already made in 1957, so that the Nobel Prize of 1981 came as a surprise. The history of this discovery is summarized in his introduction to the 9th International Conference on Electron Spectroscopy and Structures of 30 June 2003; when in a later stage specially designed synchrotrons became available as a source of radiation that could effectively excite electrons on different materials, the field expanded tremendously.

physics, not the least being Robert Maxwell—we shall discuss him later. Did Daan just happen to be the first? Probably he was, but it didn't 'just happen'. With his diplomacy, Hendrik Brinkman, the advisor with a nose for the right moment, certainly had a part in it. Hadn't he personally introduced Daan to Kai? He would do the same again, this time with a journal on instrumentation, which also enjoyed huge success. But certainly, too, Daan and Kai genuinely liked one another, and the idea of another publisher never occurred to the author. There are plenty of stories to bear this out, such as the following:⁶⁶

In the spring of 1953, Daan and his wife, Anna, were in Sweden on holiday and decided to pass by the Nobel Institute in Stockholm, on the off chance of finding Kai. They did, and promptly Kai invited them to come for supper to meet Anna-Brita, his wife. And then Anna-Brita let the cat out the bag—Kai was to be appointed in Uppsala, upon which, of course, glasses were raised. Perhaps they'd like to come to Uppsala, too? What an honour! Elaborate details followed as to the best way to get from their hotel to the Nobel Institute. But even so, they lost their way in a vast and lonely forest outside Stockholm. Daan drove fast for time was running out. Too fast; they were stopped by the police. 'Yes, I know I'm driving too fast, but in five minutes I have to be at the Nobel Institute.' 'The Nobel Institute? That's in the other direction. Drive behind me.' The policeman turned on his siren and off they went. Kai and Anna-Brita could hear the screaming siren from afar, and then they arrived at the door. And just in time, too. 'If only I had memories like that about all my authors,' Daan reminisced in old age. Kai, too, remembered the screaming siren that announced Daan's arrival. There is something of shared fate in this story.

If this doesn't explain it, then their close ties came about through Daan's intuitive doubts about the manuscript for the book on nuclear spectroscopy.⁶⁷ He had fetched the manuscript from Kai personally, spent hours going through it followed by a sleepless night. When dawn arrived, he knew what he had to do. He cancelled his flight to Amsterdam and went back to Kai with the manuscript. For even if he didn't know what it was about, carelessness and inconsistencies did not escape him. 'I knew it,' Kai said straightaway. The subject matter was alright, but pressure of work meant that he had not been able to edit

⁶⁶ *Memoir B* p. 2 in the section 'Kai Siegbahn'.

⁶⁷ *Ibidem*.

it properly. A month later he sent the edited manuscript to Amsterdam. Daan Frank as referee for Kai Siegbahn.

‘Their’ impressive handbook became the bedrock for the following collaborative project, the journal *Nuclear Instruments*. Daan was keen, after the success of *Nuclear Physics*, which we shall presently discuss, and wanted to launch this particular ‘ship’ as soon as Kai was able to muster the crew that could sail it upon the high seas. As we said, Brinkman was present at its launching.⁶⁸ In addition, the CERN survey on instrumentation, which had been discussed shortly beforehand in September 1956, at a symposium in Geneva, was the grease to lubricate the works, and could be used to fill the first issue. This would attract plenty of interest with the fine design made by the Englishman, John Adams, and his assistants—Kai, too, was impressed—and the support offered by Edoardo Amaldi and Pierre Auger, the Italian and French trendsetters of CERN.⁶⁹ But although they worked hard in Geneva to make up for lost ground in nuclear research with particle accelerators, the newest discoveries would long continue to come from the United States. To get the Americans on board, Kai went over to visit Edwin McMillan, the discoverer of the element neptunium, who was working in Berkeley (California) to prepare what was, for that time, a gigantic (6 GeV) accelerator. Without him no American would think of joining in—neither Stanley Livingstone, nor Robert Wilson, to mention the two men he had in mind. To Kai’s relief, after thinking it over Edwin declared himself prepared to join the editing team of *Nuclear Instruments*, and also suggested names to make the team truly international: these should include Japanese and, as soon as possible, Russian physicists. After McMillan’s decision, there was no point in having a separate American journal on the subject. So Kai had been in the right place at the right moment.

⁶⁸ In the Editorial note for *Nuclear Instruments & Methods in Physics Research* **500** (2003) v, Kai Siegbahn wrote: ‘[The journal] started in 1956 when I was approached by Daan Frank, the head of North-Holland Publishing Company (later part of Elsevier) in Amsterdam. He was well known as a successful publisher at that time of monographs and journals in various fields. Together with one of his advisers, prof. Henk Brinkman from Groningen, he came up to Uppsala to discuss with me the possibilities to found a new international journal covering the fast growing field of nuclear instruments and methods. The time for this enterprise was well chosen.’

⁶⁹ Interview of Kai Siegbahn on 14 November 2003; the new journal was therefore opened with an enthusiastic overview by John Adams: J.B. Adams, ‘The CERN Symposium 1956 I, *Nuclear Instruments* **1** (1957) 1–9. [‘If any conclusion can be drawn from the conference, it must be that accelerating machine projects are in no way dying from lack of nourishment.’]

'Apart from the convenience of having as much as possible of nuclear instrumentation in one journal, this instrumentation can be written up more extensively, including theory and application.' This is how Kai, the editor-in-chief, in the first number, which came out in early 1957, stimulated his readers—at a rough estimate, initially less than a thousand—to send in articles. As if he had to provide them with ideas, he added: 'What is known of the atomic nucleus today very often arises from improved technology, to make pulses of a nanosecond, for example, or more refined bubble chambers, or stronger accelerators, or energy analysers with a greater resolution.' The journal only got going slowly, with 360 pages the first year. However, in the second year, 1958, with twice as much text it was much more successful, and was published in two parts.

What man will do in the future, no one knows, but the instruments and ideas that he creates are revealing—at least it cannot be said that they have no bearing upon his future. They stand at the disposal of an organic urge to survive, but are in the process of slowly emancipating a natural urge for knowledge in a world that is otherwise empty.

From instruments, we now turn to the ideas of the nuclear physicists—their visions, explanations, interpretations and texts. We shall discuss how North-Holland, small as it was, managed to attract a large proportion of these texts and to publish them in their hundreds worldwide, around eight thousand pages each year. This success didn't just happen. In fact, *Nuclear Physics*, which appeared in the autumn of 1955 (so before *Nuclear Instruments*), long seemed unfeasible.

In the summer of 1953, when Daan consulted his advisors on why the company was taking so long to get off the ground, Sybren de Groot's possible contribution had already been discussed. Sybren may have said earlier that he didn't have time to work for North-Holland because he was busy writing a book, but now that book was finished. Suddenly he had made a name for himself, and had excellent contacts, especially in France, and this made him particularly suitable in the eyes of De Boer and Brinkman to set up and edit an international nuclear physics journal based in the Netherlands.⁷⁰ Sybren felt obliged to Daan, and when asked he said that he would be willing to try. But it was too early. Although the will to work together in the field of nuclear physics

⁷⁰ M.D. Frank, 'Léon Rosenfeld—Author, Editor, Friend', *Nuclear Physics* **A223** (1974) x; later summarized by J.K.W. (Willem) van Leeuwen in Meadows p. 260.

was strong (and here we speak only of Europe), because of its military application the will was just as strong (at least, in some countries) to keep knowledge in nuclear physics secret. This meant that CERN could only be established the following year. This is also why Francis Perrin, leader of the French *Commissariat à l'Énergie Atomique*, who Sybren knew and in 1952 had approached personally on the subject of such a journal, had to admit with regret that French co-operation could not be relied upon. The Germans, who could not yet have any military interest, also proved unwilling. Even in 1954, when they had decided to join CERN and had heard Sybren set out his plan for a collective journal, there was still one country that objected that each should have its own journals *die jeweils ein charakteristisches Gepräge zeigen* (with their own specific hallmark). But by then the French were already changing their minds.

The realisation that it would be impossible to keep secret the knowledge of nuclear fission led the Americans in 1953 to arrange for talks with Europe in the middle of the ocean, on Bermuda. That is to say, with France; for they had already shared much of their knowledge with England.⁷¹ Francis Perrin was present at these talks. From that moment he knew that the Americans wished to 'reveal all' at a conference on the peaceful use of atomic energy, shortly to be organised by the United Nations. He was also familiar with the position of the French because he had helped to formulate their response. Most certainly they would join! In August 1955 they would even reveal the recipe for the partitioning of plutonium, to the dismay of the Americans and with a nod of approval from the Indian Homi Bhabha, chairman of the Geneva conference!

In the year that he was preparing the French open position, the 'no' given to Sybren, in contrast to the 'yes' to Felix Bloch, the man who played a key role in this American stage performance of revealing all, can hardly have escaped High Commissioner Francis Perrin. Whether or not he discussed this lack of logic with Sybren is unknown, but probably not. Sybren, who must have been having his doubts on the feasibility of the journal project, had already thrown himself heart and

⁷¹ Goldschmidt p. 129; further on in this book one reads about the involvement of Francis Perrin in the French decision of 1954 to secretly develop a nuclear weapon and a nuclear submarine (p. 146), and about French views on the 1955 Geneva Conference (pp. 270–275).

soul into his new professorship in Leiden.⁷² Similarly, neither do we know when Daan went over to Manchester to pass this pioneering role on to Léon Rosenfeld. The plans for this journal appealed strongly to his old friend, even if only because it was he who had already suggested just such an idea, as he wrote to Sybren. His discussion of the project with Daan probably took place in late 1954, when Julius Podolanski was still alive, the man who was to give him editorial support. Unfortunately nothing of the agreement Daan reached with Léon is to be found in the archives. We do have a letter, however, in which Francis Perrin and Felix Bloch inform Léon Rosenfeld in 1955 that they are glad to see that Frédéric Joliot and Victor Weiskopf, and other renowned French and American scientists are to edit a new, international journal. Léon had 51 names on his list. In March CERN confirmed the Editorial Board. In April *Nuclear Physics* was announced in the widely read *Nature*, and four months later the Geneva conference began, to which 'everybody' would come.

It was a hectic summer. It began with the death of Podolanski, before he could even glance at the first manuscripts, which were coming in from all sides.⁷³ Léon, who suddenly found himself coping alone, had to ask his wife, Yvonne, to take over all the correspondence and administration, while he did the editing—a task he could not take lightly, for the standard had to be high. Of course, this could not go on indefinitely. In 1958, on his departure to Bohr's institute in Copenhagen, Gerry Brown, his assistant, would take over many of the tasks and set up a proper refereeing system.⁷⁴ Daan, too, was overwhelmed by it all. He had promised a publication time of around three months and had to search for another printer, as Holland printers couldn't possibly cope

⁷² See note 70 in this chapter.

⁷³ L. Rosenfeld, 'Obituary of Julius Podolanski', *Nature* **175** (1955) 795–796; here, the faithful assistant got the following epitaph: 'Podolanski was outstanding as a teacher; his devotion to his students was unexampled. In fact, he was incapable of refusing any request for help or guidance even when this help was given at the expense of his own investigations.'

⁷⁴ Interview of Gerry Brown on 17 March 2003: 'When I [Brown] had come to Copenhagen Léon Rosenfeld immediately needed my help in the editing of *Nuclear Physics*. He wanted to make long travels. In Copenhagen I worked together with Aaldert (At) Compagner, a young Dutch physicist who did the desk-editing. He left after a few years, however, since Léon was too often absent to advise him in the dissertation he wanted to write; later he got a doctorate in Utrecht for his dissertation *On Inhomogeneous and Metastable States*. The manuscripts for *Nuclear Physics* became numerous so that we had to expand the network of referees; we also needed secretarial assistance. At that time [1958] I had met Daan Frank already [note 43].'

with such demand. Naturally this was Dijkstra in Groningen, the only printers in the whole country who were good with formulae and still had the capacity.⁷⁵ Several years later when these printers had to expand so they could take on Daan's 180 pages per week, he bought himself into this company owned by Jan Niemeyer. Thirty years later this is what he said about it:

We didn't know what we'd let ourselves in for. We didn't even know what price to ask. We thought in terms of books and estimated the price of a subscription as if it were a book, so a price per volume with a set number of pages, not one that might turn out to be extra thick. After the announcement in *Nature* we just waited. But then they started to come in, the subscriptions, one after another, more each week, 1453 in the first year. It was a miracle. It was *the* gap in the market!⁷⁶

But how worked up he got about that competitor, who turned up in Geneva in August and chased 'everybody' to get their articles for Pergamon, so fanatically that he even jumped across one and a half metres of water to catch the departing boat for the party to which he was not invited!⁷⁷ After thirty years Daan was still seething:

Maxwell was dishonest. I don't say I was always honest, but he was really dishonest, for quite a while. Look, I was a small fish in a big pond and just wanted a bit of power. I wanted something big, where there was plenty of money to be made...to make a damned good job of something, I can do that too, I thought. I knew it had to be in the nuclear domain. And there comes Maxwell with his *Journal of Nuclear Energy*. Then De Boer wrote to Maxwell—⁷⁸

⁷⁵ *Memoir B* p. 3 in the section 'De druk van het drukken' (*The pressure of printing*).

⁷⁶ Interview of Pieter Bolman on 11 September 2003. [The quote is, of course, to the best of Bolman's recollection; his discussion with Daan Frank about 'the miracle' of *Nuclear Physics* took place around 1980.]

⁷⁷ Elisabeth Maxwell (*Robert Maxwell & Pergamon Press*) pp. 241–242; the author, Michael Williams, tells the following: 'The various delegations at the Geneva Conference vied each other to give the most magnificent receptions and, of course, Bob Maxwell attended all of these (either invited or uninvited) to make his contacts. In one instance, the French decided to hire a boat and serve a meal whilst sailing around Lake Geneva. The numbers for this event had to be limited and invitations were carefully scrutinized as people came aboard. As the boat pulled away from the quayside and there was about five feet of clear water, Bob ran to the edge shouting "wait for me" and jumped across. The sailor who helped him was too concerned for his safety to check his invitation—rumour has it that he did not have one and this was a ruse planned well in advance.'

⁷⁸ Cited from the taped interview of Daan Frank by Han Kruyswijk and Pieter Bolman on 15 November 1985.

Here the recording of this interview from 1985 breaks off and the letter it refers to cannot be recovered. Jan de Boer, who we were still able to speak to, said that he never wrote to Maxwell. It must have been another De Boer. Fortunately we know from another source why Daan was so angry. In the gigantic book Elizabeth Maxwell devoted to her husband as a publisher, we read that 'Bob' had got authors to sign up to write books and edit journals (exclusively) for him, and that he was so successful in this that 'the Secretariat of the United Nations included a paragraph in one of their daily bulletins, in which stood that the Proceedings would be published by the United Nations, and not by Pergamon!' So Maxwell must have made 'everybody' think there in Geneva that he was acting on behalf of the conference organisation. If we further consider the fact that the chairman of this conference, Homi Bhabha, was nominated to be an editor of *Nuclear Physics*, and that the name *Journal of Nuclear Energy* was very similar to *Nuclear Physics*, then we can understand why Daan Frank called Maxwell very dishonest.

Daan's fear that the *Journal of Nuclear Energy* would spoil the market for *Nuclear Physics* was not entirely groundless. It's clear to us, fifty years later, that *Energy* and *Physics* point to different markets. The first is applied technology, and the second fundamentally scientific, and it was no coincidence that the chief editorship of the first journal fell to an engineer (of the British Atomic Energy Authority) and the second to a scientist. But for Daan the line between applied and fundamental knowledge must have seemed vague. The success of *Nuclear Physics* depended, therefore, on quality, a quality that was guaranteed by institutional support. The article by two CERN scientists, Bernard d'Espagnat and Jaques Prentki, which appeared in the first issue of October 1955, immediately attracted a lot of attention.⁷⁹ The article was so often quoted that it gave the journal a favourable position in the market, more so than the somewhat predictable opening note by Léon Rosenfeld in the first issue, though this too was not without its merit.

We almost drown in the flood of new results of experiments with particle accelerators, results that are often not understood and set theorists feverishly to beat their brains, he begins. So while there is much to be exchanged between the various research centres, the information and ideas, what it is actually about, is hardly to be found amongst the

⁷⁹ B. d'Espagnat and J. Prentki, 'Formulation Mathématique Du Modèle De Gell-Mann, *Nuclear Physics* **1** (1956) 33–53.

‘vast stacks of periodicals of every kind of origin.’ By this he means national academies and laboratories. To quote Léon word for word: ‘The fact that physicists are sending preprints to colleagues following the same trail means that the periodicals no longer properly fulfil their function. The publication of original articles in the diverse fields of physics will have to be more rationally organised. This new journal is an attempt to do so. It is devoted to the experimental and theoretical study of the atomic nucleus, bearing in mind that the nature of nuclear forces has yet to be discovered, with attention, therefore, for the quantum field theory on the one hand, and on the other for the ‘elementary’ particles.’ Finally he stresses the international character of *Nuclear Physics*, ‘in which editors are represented from all countries or groups of countries where nuclear research is carried out.’

The first volume of *Nuclear Physics* contained 690 pages. Already this was one fifth of the approximately 3800 pages that *Physical Review* devoted to nuclear physics in the same period (from October 1955 until September 1956). The difference would almost disappear altogether. In 1964, when *Physical Review* went on to publish nuclear physics articles in a separate series, this B series contained exactly 6376 pages. That same year *Nuclear Physics* had 7745 pages, although with each page containing somewhat less text. We shall later discuss the growth of this journal in the 1960’s and the crucial role of Gerry Brown.

But there is something else to discuss first. For how big, in fact, was this gap in the market that Daan saw? In 1956, when he discovered that there was plenty of space for a journal like *Nuclear Physics*, he undoubtedly saw an even bigger gap. After all, that same year he’d spurred Kai Siegbahn to launch *Nuclear Instruments*. Might he not have thought that there must be even more space ‘within the nuclear domain’ in which he could find a niche? A man who has once said that he wants power, as soon as he has tasted it, will want more. This is the first law of power. He already knew Kai (and Léon, who had also been indispensable to him), but no one yet who could lead him any further in the nuclear domain. In 1957 he had every reason to curse Maxwell, because a sizeable number of authors must have said to him that they were committed to Maxwell, because of their signature. Nevertheless, he managed to find an excellent candidate in a lecturer from Birmingham:

One day in the beginning of 1958, when I was 33 years old, I got a letter from a certain Daan Frank, the owner of a Dutch publishing house, North-Holland Publishing. He suggested that I become editor of a new science journal that was to be called *Journal of Nuclear Materials*. It was to

cover all materials, not only metals, which are used in nuclear reactors and in nuclear fuel vessels, coolants and moderators. This idea was new and stimulated me sufficiently to get to know more. Mr Frank invited me to his office in Amsterdam, and took me for a meal in the Havengebouw, a beautiful new building with a restaurant on the top floor, in the middle of a vast harbour. He spoke very convincingly on the merits of his idea, and most flatteringly of my suitability for the job. I never did find out who had suggested my name. This was my first experience of a publisher's specially geared hospitality. Since then I have had ample experience with publishers, a very personal hotchpotch, who observed all kinds of approach to hospitality—one of them wanted to share the bill with me for a lunch we had together! Daan Frank was not only convincing, but also had an old-fashioned courtesy and charm that was irresistible, and so I took upon myself the extra job, on top of my teaching and research. Later that year Frank and I attended the second international conference for the peaceful use of nuclear energy, in Geneva, and there we persuaded Paul Lacombe to be fellow editor. I knew his name; he was a competent metallurgist from the École des Mines. Frank helped me break the ice, because the French are always a bit suspicious and I didn't know how to go about approaching him. Shortly afterwards I found an experienced American metallurgist and physicist from General Electric, John Howe, who wanted to be the other fellow editor.⁸⁰

This is how Robert Cahn begins the chapter in his *Memoir* that deals with his work as science editor. Originally a German Jew, born in 1924, he fled with his mother to Majorca six months after the Nazis seized power. At the outbreak of the Spanish civil war they gradually made their way, with many detours, to join his father who was a businessman in London. The war, which caused such destruction in England as well, made him British, very British, and in 1947 he obtained British citizenship. He went to Cambridge, got his PhD in research into polygonisation in metals at the Cavendish Laboratory, and worked for a time under John Cockroft at the establishment for atomic research in Harwell; then in 1951, and still young, he became a lecturer at Birmingham University. In his research into the origin of the strength and plasticity of the new materials that were necessary to build nuclear reactors, he had discovered several remarkable ways in which atoms can regroup in crystals. This, together with the clear summary that he gave of it in *Advances in Physics*, would in 1962 make him the first professor of material science in England. The subject was new, because new metal alloys and semiconductors as well as oxides were being discovered, and

⁸⁰ Cahn pp. 103–104.

the man was new—an enthusiastic immigrant who wanted to make a success of his life overseas.⁸¹

We looked him up in Cambridge, this Fellow of the Royal Society, already long retired and having written several books. Was it a coincidence that he offered us lunch with oysters and wine?⁸² As science editor clearly he had served Daan Frank extremely well; but how did they manage to discover one another? This question is by no means insignificant if we wish to understand the development of North-Holland beyond Daan's domain in Amsterdam. He mentioned Cockroft, but immediately shrugged his shoulders. He didn't know. We can only guess. Who brought Daan to Cahn? Not Hendrik van Bueren, although that seemed a possibility. Later he was to join Daan's editing team, and visited Daan in 1957 to discuss the publication of *Imperfections in Crystals*, a book he wrote that describes a considerable amount of Cahn's work, as well as the effects on materials (imperfect crystals) by radiation from atomic nuclei. But when we asked him he denied it; it was more likely that Daan had brought him, Hendrik, to Cahn, to check his English in his book. Another possibility is Paul Rosbaud. We think, in the end that it was he, although the secrecy with which this man surrounded himself his whole life makes it impossible to verify. We know that in 1957 Rosbaud offered his services as advisor to several science publishers, after breaking off his contract in September 1956 with Pergamon, embittered at the way he had been treated by Maxwell after managing his company for five years and building up an excellent list.⁸³ It is very likely that he approached Daan as well in 1957. Certainly he knew him well in 1960, when he and Daan visited Cahn to discuss a follow-up project.⁸⁴ Did Daan feel something of triumph when he

⁸¹ Cahn pp. 13–46, 61–82.

⁸² Interview of Robert Cahn on 1 March 2004. Before lunch he showed books written by scientists on the history of their speciality, and praised in particular the one by Seitz and Einspruch (see Bibliography); these authors really understood their subject, which can seldom be said of historians of modern technology. For this reason, he said, all reviewers had welcomed his own historical book [*The Coming of Materials Science*, Pergamon (2001)]. He also had a much read column in *Nature*, of which the pieces were later published in *Artifice and Artefacts* [Institute of Physics Press, Bristol (1992)]. During lunch we exchanged views on the life of a physicist who at the same time is a writer.

⁸³ Haines pp. 166–168.

⁸⁴ Interview of Robert Cahn on 1 March 2004: 'In 1960 Daan Frank came along with Paul Rosbaud to convince me [Cahn] that a book like *Physical Metallurgy* was necessary and that I should edit it. It took five years before this collaborative effort of 25 authors was ready. It had much success, so that after another five years a second,

wrote out a cheque for Paul Rosbaud for this service? Certainly his subterfuge had bought him precious know-how from Pergamon, the dishonest competitor.

The *Journal of Nuclear Materials* contained a foreword not only by the editors, but also by High Commissioner Francis Perrin, who wished to stress the French interest in it. Together these determined its place among all the current journals on material science, a place that had to be fought for. In 1959 there was copy for only 386 pages, and in 1960 for only 364 pages, but in 1961 it became more successful with 721 pages, and in 1962 with 1077 pages. It served as good training in assessing articles for the young Robert Cahn and—certainly in the beginning—in drawing them in. You couldn't do it, he said, like that retired military serviceman who once edited a journal and just carried out to the letter what the referees suggested. For him these were just orders and he had no idea whatsoever what the journal was about. But Robert was unable to say how that capacity to assess an article grew, an article which might be good but most often needed improving, and sometimes even had to be refused, a capacity that gradually became more acute. 'The patient must heal himself!'⁸⁵ For Robert, all the work that he did during 25 years on the *Journal of Nuclear Materials* would mean a useful contribution in his own field. The same applied to his editing of *Physical Metallurgy*, a handbook with several authors and cause for another visit from Rosbaud and Frank in 1960, but which would not be published until five years later by North-Holland.

slightly revised, edition was necessary (1970); the third and fourth edition (1983 and 1996) were substantially revised and enlarged with the help of Peter Haasen. The last edition with its three volumes of 1000 pages each was a blockbuster. It really had become my *Magnum Opus*.'

⁸⁵ Cahn p. 105.

CHAPTER SIX

BEYOND PHYSICS

So what happens next? We are deluged with events. Our little driver caught in a cloudburst provides an apt illustration. The drumming rain has turned into a thunderous roar. His Wolseley, hardly a featherweight, is thrust sideways by treacherous gusts. He slows down, turns on the lights. All those tons of water pouring down onto the dark countryside around him—he must save them from insignificance. The water streaming over the windscreen comes down faster than the wipers go back and forth—he will just have to make do with the rare seconds when he can see the road ahead. Look how the red rear lights in front of him skate from side to side...and then another great wave, from the traffic in the opposite lane, lands with a thud on his bonnet. Now he understands. This is physics.

Past Halfweg on the way to Amsterdam, the weather clears. The little driver thinks to himself: ‘I should have stopped, but I carried on. I’ve been lucky. I am a lucky man.’

And we spectators, we too have arrived at the Keizersgracht in Amsterdam, and stop in front of a large office building on the corner of the Wolvenstraat. At the end of the tumultuous sixties, just as the young are bringing the city into uproar, the property is being renovated, redecorated and provided with new facilities for around eighty editors, typists, bookkeepers, designers, salespeople and shippers. At long last the board of North-Holland Publishing Company has ventured into this first-class location in the city: the previous premises on the Nieuwezijds Voorburgwal has outlived its purpose and can no longer cope with such rampant growth.¹

The growth of North-Holland in the sixties can best be seen as the direct result of the success of *Nuclear Physics*. We have already mentioned its stake in Dijkstra, the large printing works in Groningen, and the appointment in 1956 of Wim Wimmers, the physicist, as desk-editor

¹ Daan Frank’s unpublished papers, *Memoir B* p. 1 in the section ‘1961–1970’.

of *Nuclear Physics*.² When Wim was able to hand over this task two years later to an editor stationed in Copenhagen with Rosenfeld, he was sent out to find authors for the physics list. This was necessary, as few remained in the pipeline. Daan Frank had done the ‘cheap’ publication of the *Collected Scientific Papers* by Hendrik Kramers, as well as his pre-war book on quantum mechanics, but apart from this he no longer had time—extending his lists in other disciplines must take precedence. Wim had no experience in acquisition, and a certain stiffness and reticence made him seem less suited to the job.³ But he was also tough and indefatigable—qualities that had stood him in good stead in the bush of Sumatra, where he carried out research for an oil company, and perhaps even more so in the classroom, where he taught elementary physics for several years.⁴ Now, at 38, he seemed to have discovered his true calling. He became a nomad. A comfortable Dodge conveyed him to the bare hotel rooms where each evening he wrote out his reports. Meanwhile he was still able to keep up with all the specialists suggested by the advisors of North-Holland, and to negotiate their contracts. When he retired this is how he described it:

I just rolled into the job, but I quickly learned what it involved: talk with as many people as possible, go to congresses where ‘everybody’ is to be found and pick up what a publisher needs to know: reputations of journals and researchers, new developments, and so forth. Then ideas come of their own accord. An acquirer of manuscripts must come up with ideas. But be aware, however much he may know, and however much financial leverage he may have been given by his boss, it is never certain whether his idea for a book or a journal will actually turn out to be a good one, and lead to a profit. He is out there on his own, a lone wolf, and must bite when the moment is ripe. I remember being at a congress in Evanston,

² Idem p. 1 in the section ‘Van Tongeren, Wimmers, Krips & Baltzer’: ‘The launch and rapid growth of *Nuclear Physics* forced [me] to appoint a desk-editor, and W.H. Wimmers, the man that applied for this job after the advertisement in *Nederlands Tijdschrift voor Natuurkunde*, presented himself as very serious, hard working and dedicated. In fact, he performed so well as desk-editor that I could promote him to acquisition editor in a year or two.’

³ Idem p. 2 in the section ‘Van Tongeren, Wimmers, Krips & Baltzer’: ‘In the course of time we became accustomed to his [Wimmers’] peculiarities. He didn’t grouse but didn’t wear his heart upon the sleeve either. A solitaire he was, with great qualities.’

⁴ Interview of Wim Wimmers on 11 March 2003. By way of introduction the 83-year old man showed a unique drawing of the so-called Fermi pile [the world’s first nuclear reactor, built in 1942 in Chicago] that was “presented to dr Willem H. Wimmers from his friends at Argonne National Laboratory in appreciation of his contributions to the advancement of nuclear energy.”

near Chicago, when I was just starting. I was talking to this interesting fellow about setting up the editing of a new journal, and after a bit he said, 'Ok, I accept the conditions, shall we sign then?' It was something that I should have discussed with the boss, but that would have given such a poor impression. You have your pride to think of. So I signed, on behalf of North-Holland, and it turned out well. But there are risks. Not only financial ones. An editor may turn out to be a disappointment, or no longer live up to his obligations as a scientist. Then he has to be dismissed. I've had to write quite a few of those letters, which is hard, and of course, if you do, you must be sure of a successor.⁵

And he was good at his job. Just in his first seven tenacious years, between 1959 and 1966, Wim reaped a harvest of 38 books on physics, including four volumes of *Progress in Optics*, a series edited by the renowned Emil Wolf.⁶ Wolf's name came from Paul Rosbaud and, according to Wim, without all his advice to Daan Frank, it is doubtful whether North-Holland would ever have achieved its international reputation.⁷ This last is sheer nonsense—as anybody who takes a look at the development of the publisher's list of North-Holland can see.

The success of *Nuclear Physics* had other less obvious consequences: besides increasing acquisition and printing capacity, it also stimulated innovation. Léon Rosenfeld had quickly supplemented his journal with a letters section, following the example of *Physical Review*, to be published within one month, just as with *Physical Review Letters*. This meant that Daan was forced to seek out fast production methods.⁸ Only once had Theo Geuzenbroek, his production assistant, managed to produce the letters within one month using the usual method of hot metal typesetting, i.e. lead type. But strictly speaking this was not feasible, especially

⁵ This text is adapted from 'Handel in kennis' ('Trade in knowledge')—interview of Wim Wimmers by Rob Biersma in the newspaper *NRC Handelsblad* of 1 April 1982.

⁶ Emil Wolf, born in Prague in 1922, had to flee from his fatherland in 1938 and eventually arrived in Bristol, where he studied physics. In the early 1950s he worked in Edinburgh and wrote there, together with Max Born, the *Principles of Optics*. The fact that this book was published by Pergamon Press, then (in 1959) still in London, makes clear that Paul Rosbaud knew him. The authoritative status of the book was immediately recognised and gave Wolf access to a professorship in Rochester (U.S.A), where he later predicted a cosmologically important frequency shift of light that is not caused by a motion of the light emitting source (*Physical Review Letters* **56** (1986) 1370–1372). The series *Progress in Optics*, published by Wim Wimmers for North-Holland, started in 1961, contained excellent reviews (for instance *Light and Information* by Dennis Gabor) and sold well—some volumes had to be reprinted.

⁷ See note 4 in this chapter.

⁸ Daan Frank's unpublished papers, *Memoir B* section 'Physics Letters & Northprint BV'.

if the referee and the post took another two weeks. In 1962 Rosenfeld had already resigned himself to the delay, when Wim Wimmers came up with the idea to publish the letters separately, to gain time, as speed was essential for a letters journal. Then Daan remembered that there was an offset printing company for dissertations on the Stationsstraat in The Hague, where the text was not set, but typed, then photographed and mounted onto a rubber roll, which was the new method of cold type and offset printing.⁹ In 1953 he had once had a thousand copies of *International Trade under Flexible Exchange Rates* printed, a book of 348 pages by an American who happened to walk into his office. It looked good, but required a lot of time, and could only be approved after a third printing. Even so, typing must be quicker than typesetting! Daan sent Wim along to the printers. We quote Jan Krips, son of the owner, who had typed more than 360 dissertations from the time he was 14 years old:

In July 1962 Mr Wimmers came and fetched me in his Dodge, as I had two typewriters to carry. On the way to Amsterdam there was a very rudimentary job interview, and he asked, 'How much do you want to earn?', and I said, 'Four hundred and fifty guilders.' Nothing more was said of the matter. I didn't say much because my father wanted to close down his composing room and didn't want to accept any more typing work. He just sent me to Amsterdam to do what North-Holland had asked him to do. In 1962 in my father's printing business we didn't yet use typewriters with exchangeable heads. We had Type-It, an IBM system with plastic rods, springs and metal letters. The whole construction was rather weak, because each rod had to be inserted in a different manner into the visor to get a good letter print. We also had a machine with Greek letters and mathematical symbols for the dissertations. This is how we used to type out a page, together with four other machines (two for non-standard letters and their italics, and two for small standard letters and small italics), leaving a space every time there was a letter that was not on the machine. Fraction bars, large brackets and other such symbols we drew by hand. It wasn't that simple, working with all these different machines, but until IBM brought out the Composer with exchangeable heads in 1969, we had nothing else. This is how I typed the first batch of *Physics Letters*. Professor Gerry Brown wanted to see it before it was printed. As I didn't possess the means to copy it, I sent the original typed version to him in Copenhagen. After a week he sent it back with the excuse that his children had got hold of it. So I had to start all over again.¹⁰

⁹ Idem pp. 2–4 in the section 'Preludes to the Krips story'.

¹⁰ Letter from Jan F. Krips to the author of 28 June 2003.

Wim couldn't believe that Rosenfeld, who set such store by elegant typography, would agree, but Brown, who had succeeded him in this particular task, found this feat of Jan Krips to be 'ingenious'.¹¹ It is a good example of innovation in the kind of dexterity that could be learnt—just like playing the piano—and which took advantage of the high speed of electric typewriters long before the technique of exchangeable heads was developed. We shall explain why it was called *Physics Letters* and not *Nuclear Physics Letters*, which would seem a more obvious title for these letters, but first we must say something about Gerry Brown.

Gerald E. (Gerry) Brown, tall, tousle-haired and unconventional, came from a small town in South Dakota, where he was born in 1926, son of those vast wastes of the Mid-West of America, so unimaginable to West Europeans.¹² He obtained his degree at Yale University in New Haven (Connecticut) when he was 24 years old. Leftist leanings drove him from the country at the time when Joseph McCarthy and his reds-under-the-bed brought such confusion and dismay. In 1955 he was appointed lecturer in mathematical physics at Birmingham University (England), where he came into contact with Léon Rosenfeld, who greatly inspired him. Two years after Léon left for Copenhagen, he was able to join him at the *Nordisk Institut for Teoretisk Atomfysik*, as a professor. Here, in the 1960s, he would discover the Kuo-Brown force in the atomic nucleus, and later the Brown-Rho scaling law.¹³ He would come into conflict more than once with the authorities—in this case with Tsung Lee and Chen Yang, the Nobel Prize winners of 1957, who found his idea of a force that holds quarks together in a 'bag' nonsensical. But *NordITA* was the perfect place for him, and he loves to show the letter of appointment signed by Niels Bohr himself, in Danish, on 7 July 1960.¹⁴ We looked him up in Stony Brook near New York, after

¹¹ Interview of Gerry Brown on 17 March 2003.

¹² Gerald E. Brown, 'Fly with Eagles', *Annual Review of Nuclear Particle Science* **51** (2001) 1–22; see in particular p. 14 (on the Kuo-Brown force) and p. 16 (on the Brown-Rho scaling).

¹³ See previous note.

¹⁴ Begin and end of Bohr's letter to Brown at Moseskillet 8, København:—*Af styrelsen for Nordisk Organisation for Teoretisk Atomfysik er jeg blevet bemyndiget til at tilbyde Dem fast ansættelse som professor ved Nordisk Institut for Teoretisk Atomfysik i København fra 1. juni 1960 at regne. Efter stedfundne mundtlige forhandlinger med Dem er der opnået enighed om følgende betingelser for ansættelsen [...] Jeg vil gerne bede Dem bekræfte at have modtaget ansættelsen på de ovenfor nævnte betingelser ved at underskrive og returnere genparten af nærværende skrivelse. København, den 7. juli 1960—Niels Bohr, formand for styrelsen.*

he had returned to America at the end of his career in Europe. 'At first my article on the Kuo-Brown force was refused,' he said. 'It was almost impossible to get it published. The referees treated me badly. They failed to see that controversy lies at the heart of science. They should have asked questions about it. That's what I did when I edited journals. Never turn down an article; turn down the version.'

Gerry had only just arrived in Copenhagen when Léon Rosenfeld announced that he intended to travel, and that he, Gerry, would have to take care of *Nuclear Physics*. He might as well forget doing any research, it seemed. To sweeten the pill, Daan Frank, the 'enlightened capitalist', who had a great interest in the journal and therefore regularly visited Copenhagen, impulsively wrote him out a cheque for twenty thousand dollars so that he could buy the country house his wife was so keen to have. 'He wanted a sort of family relationship.' So when Wim Wimmers asked him in 1962 to be editor-in-chief of a letters journal, caught as he was between friendship on the one hand and obligation on the other, he could hardly refuse.¹⁵ It was to be called *Physics Letters*. For Samuel Goudsmit had recommended that the journal have a general, not exclusively nuclear physics, character. This editor of the *American Physical Review Letters*, who originated from the Netherlands, welcomed such a journal. 'Let the Europeans publish their own garbage' was what he said, according to Wim. Gerry said 'no', however, to the idea that he would also edit letters on non-nuclear subjects. This part of the editing was then entrusted to a lecturer from Oxford, Dirk ter Haar, with whom he maintained a rather difficult relationship.¹⁶ In his eyes ter Haar made two unnecessary demands: that the letters should be no longer than one and a half page, and that they should not be looked at too critically. The latter was quickly justified, however, by ter Haar's decision to publish a letter that was refused by Goudsmit, in which a 22-year-old student predicted that an alternating current

¹⁵ This success has three fathers. Both Wim Wimmers (note 4) and Gerry Brown (note 11) claim that it was their idea, while Daan Frank writes (*Memoir B* section 'Physics Letters and Northprint BV'): 'The question was whether the speed of type-setting by Jan Krips would allow us to beat the *American Physical Review Letters*, by having just two weeks between the receipt and the publication of a manuscript. Krips said he could manage it if other work be skipped, Wimmers saw problems with the quality, but [the provisional editor] Rosenfeld had no such reservations. Then everything was decided in a hurry. Only in setting-up an editorial board we were a bit delayed by obstruction of Sam Goudsmit, who feared the competition with 'his' *Physical Review Letters*. Later on Sam and I became friends.'

¹⁶ Interview of Gerry Brown on 17 March 2003.

must run between two weakly coupled superconductors when a constant voltage is applied across a thin insulator separating them. That was Brian Josephson. Eleven years later he was awarded a Nobel Prize. Talk of ‘garbage’! This prediction in *Physics Letters* **1** (1962) 251–252 immediately attracted interest, but the abundance of letters on nuclear physics that Gerry managed to obtain from Jacques Prentki and other CERN friends made an overwhelming impression.

We shall not discuss the further development of this letter journal, the first ‘quickie’ in Europe, its division into A (for the non-nuclear subjects) and B (for the nuclear), and again B into B (for the low energetic processes) and C (for the high energetic), and the subsequent *Physics Reports* that led on from C to be edited by Maurice Jacob in Geneva as accessible summaries of the physics of ‘elementary particles’. Nor shall we further discuss the production process of this ‘quickie’, which entailed the training in competency and speed of more and more typists, as well as the use of newer and newer, and in the end computer driven typewriters at Krips Repro, a large, independent printing works in Meppel.¹⁷ These developments are self-evident. We do better to concentrate on the third outcome of the success of *Nuclear Physics*, namely the opportunity to strengthen the management of North-Holland with a professional publisher/manager and the necessity thereof.

Excerpta Medica

Engelbart (Bart) van Tongeren came from a small but worldwide publishing company for medical extracts. In 1962, when he was asked to join the board of North-Holland, he had succeeded in making this originally insolvent company into a profitable concern, but not in keeping the profits out of the shareholders’ hands. But this is a story for later on. He was 42 years old when Daan Frank asked him to become co-director of North-Holland. For a second time, his life took a fortunate turn, and he accepted the offer with both hands.

Bart had wanted to become a civil servant in the East Indies.¹⁸ His sonorous voice and solid build were undoubtedly suited to the job, as

¹⁷ Daan Frank’s unpublished papers, *Memoir B* section ‘*Physics Letters* & Northprint BV’; Letter from Jan F. Krips to the author of 28 June 2003.

¹⁸ Interview of Bart van Tongeren on 12 February 2002—the first interview for this book, during which Van Tongeren spoke for hours about his career at Excerpta

well as his ambition to get ahead, far from the modest Amsterdam milieu in which he was born in 1920. During his studies on Indonesian life and culture, paid for by the state—without this support he would not have been able to attend university—he learnt Malayan, the *lingua franca* of the inhabitants, as well as the constitutional law, cultural anthropology, history and economics of what was then the Dutch East Indies. The brilliant lectures of Frederik Gerretson on the history of this colony and the multinational trading enterprise (the Dutch East Indies Company, the VOC), which was its predecessor, made a deep impression on him. That the East Indies would gain independence and no longer need the white administration was something that could not have been foreseen in 1942, when he took his doctoral exam.

At that time it was impossible to leave the Netherlands and more or less as a matter of course Bart joined the Resistance. In his case this meant working for the underground newspaper *Het Parool*, one of the three illegal newspapers with large circulations that were distributed right across the country in spite of the great risks involved. He worked together with Frans Goedhart (Pieter 't Hoen), Wim van Norden, Simon Carmiggelt, Jan Meijer and Herman Sandberg. And what happened to many, happened to him too. In July 1943, he got caught when he rang the bell at a house. Not the Jewish woman whom he had come to fetch, and who was supposed to get a place on an escape route to Switzerland, but a German policeman opened the door.¹⁹ He was arrested. However, he stuck so well to his story of why he was ringing at the door, that the German security police could find no proof of any illegal activity and released him after three months. He got away with his life, but Wilco Jiskoot, his companion in the Resistance, didn't and was shot before his eyes. As for the rest of his experience in the Resistance, Bart only let on that he took on the pseudonym 'Philip'—a name that he passed on to his son—and that from *Dolle Dinsdag* (crazy Tuesday, 5 September 1944, when, after the fall of Antwerp, the Dutch believed they were about to be liberated) until the actual Liberation (5 May 1945) he organised the production and distribution of the thousands of copies of Pieter 't

Medica, North-Holland and Elsevier, with emphasis on the *happy* years he was with North-Holland; on a second meeting (7 March 2002) he started to provide documents on the history of these companies, and from then on he always enclosed such documents in his letters to the author.

¹⁹ Letter from Bart van Tongeren to the author of 20 April 2005.

Hoen's daily news bulletin.²⁰ The German retreat meant that he could now leave for Batavia (Jakarta), where the Dutch government sent him in August 1945 to begin work as a civil servant.

His journey took him first to Trafalgar Square in London where, in all the commotion of the celebrations at the Japanese capitulation, he bumped into Pamela Woodland—later to become his wife—and then on to Newcastle where a brand new ship with only first class accommodation was waiting to sail to Tandjung Priok.²¹ But in Batavia there was no work to be had. There was little to be administrated in the chaos of the Japanese retreat and the beginnings of guerrilla warfare on the part of Indonesian freedom fighters, while the Dutch had only just been released from the internment camps. Bart was sent to the more settled Makassar in the Celebes (Sulawesi) in the east of the archipelago, where he was posted to a government information service. Here he returned to working order a damaged printing works for a newspaper, for which he had to learn the technique of lead setting, and then also a radio station, for which he did the communiqués. Like so many young people at the time who were dropped into the tropics, he had to learn how to cope. He enjoyed the freedom enormously, but his salary was next to nothing. It was nowhere near enough to support a family—and he wanted a family, for by now Pamela had joined him and they were married. To increase his income, in 1947 he started a monthly publication, similar to the *Readers Digest*, in Malayan, under the name *Tjerman Doenia* (Mirror of the World). This earned him a good income until the beginning of 1950 when, six months after handing over sovereignty to the government of the Indonesian Republic, it became clear to him and other Europeans that there was no future for them in the archipelago. When he left, the money that he had earned from his business lost 5/6th's of its value, due to financial measures taken by the Indonesian government.

Penniless and back in Amsterdam, someone tapped him on the shoulder in the Kalverstraat. 'Hey, Philip!' It was a friend from the Resistance. After catching up on each other's news the friend said he knew of something for him: *his* job. He'd just handed in his notice. It was the post of executive secretary of Excerpta Medica, a small publishing firm located above a shoe shop in the same Kalverstraat.

²⁰ Jong **10B** p. 448 (see also the preceding reproduction 78 of *Het Parool*).

²¹ Interview of Bart van Tongeren on 12 February 2002.

Bart could start straightaway, if he wanted to. And he certainly did. On the first day of his becoming secretary, in the autumn of 1950, not a member of the board was to be found, just a bookkeeper who stuck his head around the door after a few hours and informed him how the salaries were to be paid at the end of the month... We let Van Tongeren continue:²²

When I began at Excerpta Medica, a limited liability company, it was practically bankrupt. In 1947, when it was set up, it had received a capital of 300,000 guilders, and in the meantime it was a million in the red. It was only thanks to large loans from the Nederlandse Herstelbank and an American financier—because of its international character—that there was a small amount of cash on hand. Nor was it a Dutchman, but a German Jewish refugee, Erich Landsberger, who took the initiative to set up the company, acting on the suggestion made to him by the Jewish doctor Janos Freud: Summaries of all important medical publications from across the world should be published in English. Such documentation already existed in German, in the form of the *Zentralblätter* published by Springer, but Landsberger was convinced that Germany would be beaten and the *Zentralblätter* would go down, too.²³

During the war, while in hiding in Amsterdam, Landsberger had found some support for his plan from a few Dutch professors and also a few publishers, namely Frédéric (Fred) von Eugen, Fritz Landshoff and John Meulenhoff. The country had only just been liberated when he rented rooms in the Kalverstraat in Amsterdam and began to set up editing teams for all the specialties that Freud had suggested to him. An international network of professional experts was to be assigned to each editing team. It wasn't until the end of 1947 that he was ready and Excerpta Medica ("by the medical specialist, for the medical specialist") could be set up. The publishers we have mentioned were the owners and formed the business management, Landsberger became director, while James Cauv-

²² Letter from Bart van Tongeren to the author of 24 July 2004; this letter has 18 pages and addresses various subjects—the quote is from p. 12.

²³ Fredriksson p. 163 [in the Chapter by Robert R. Blanken & Pierre J. Vinken where, prior to the present publication, a few lines are devoted to the history of Excerpta Medica]. In the recent Vinken biography by Frentrop lines are also devoted to this history but this book appeared too late to take them into account in this volume. In a letter to the author of 17 August 2005, Bart van Tongeren gave details about the *auctor intellectualis*: 'Janos Freud was born in Hungary in 1901, studied medicine in Vienna, then went to Paris, London and Cork for a specialisation in endocrinology, and finally settled in 1935 in Amsterdam. Here he found a job in the Pharmacological Therapeutic Laboratory and also a non-Jewish wife, Lini Versteeg. The 'mixed marriage' saved him from the Nazis, but his fears as a Jew in occupied Amsterdam and his conviction that Germany would lose the war, brought him to the plan of an English alternative for the German *Zentralblätter*. He died in 1947, shortly after the publication of the first volume of *Excerpta Medica*.'

erien and Peter Warren, an English colonel, were appointed as executive secretary and sales manager, respectively. Martinus Woerdeman, professor of anatomy in Amsterdam and member of the Royal Academy, was chairman of the editorial board. One year later, shortly before the death of Janos Freud, the first journal was published, number 1 of a series of 12: *Dermatology and Venereology*. It was a great achievement, but the losses in starting it up were enormous.

As I said, the firm was in a precarious state in 1950. Because there was as yet no profit and also because the threat of bankruptcy might affect their other publishing businesses, in 1952 Von Eugen, Landshoff and Meulenhoff assigned the business management of *Excerpta Medica* to Peter Warren and myself. They still remained owners and directors. I venture to say that it was largely due to my management that there was positive capital again, 1,254,898 guilders to be precise, at the close of the financial year 1961, just before I switched over to North-Holland. After having to accept an initial loss of 0.3 million, I was able to turn the tide, and make a profit of 2.5 million between 1954 and 1962. It could have been even bigger if the owners had not been so acquisitive... Fred von Eugen was constantly egging me on “to greater achievements”. It was almost insulting: “Isn’t the board pulling out all the stops?”, “Do you know what it costs per hour?”, “Ah, so we want to buy a house, do we?” But I have to be honest—I did admire the man. The way he got hold of that million guilders we had to borrow, and the way he managed to have us calculate a higher dollar exchange for the export—not many could have done what he did. Without Von Eugen, *Excerpta Medica* wouldn’t have stood a chance.²⁴

So how did Van Tongeren manage to turn such losses into such a profit?²⁵ Briefly, when it became clear that the manner in which the medical editors and their experts wished to work—painstakingly, labour-intensively and therefore costly—he decided to economise on the printing instead. When the excerpts could be printed after a typical editing period of a year, this was done in lead. Neither Von Eugen nor Landshoff nor Meulenhoff, all book publishers, had realised that an alternative existed for lead (hot metal type) for small editions like *Excerpta Medica*, namely setting on a typewriter (cold type), and then offset printing. This was far cheaper. So Bart bought six up-graded Vari-type typewriters, and from the beginning of 1953 had all the journals typed by in-house staff, and then printed by Henkes, the offset printers

²⁴ Letter from Bart van Tongeren to the author of 24 July 2004.

²⁵ Bart van Tongeren, *Medische Referaten (Excerpta Medica) N.V. 1947–1961*, unpublished paper of 9 pages that was presented to the society ‘Nonpareil’ in Amsterdam in 1999, p. 4.

in Haarlem. This cold-type production of a whole journal list with an edition of a thousand copies had never been done before. (Here we should point out that the previously mentioned thousand copies that Daan Frank had printed in offset by Krips in 1953 was a *book*.) For the first time, in 1953, there was no loss.

However, not only was keeping down costs a problem, but earnings lagged behind as well. Here again the conservatism of Von Eugen, Landshoff and Meulenhoff played a role. They had put foreign sales into the hands of sole agents, and promised them a 50% discount—a percentage not unusual for booksellers when there was a substantial purchase of a new book within a short time. These sales contracts, however, stated no minimum purchase, apparently because they expected a high turnover for *Excerpta Medica*. An elated Von Eugen had telegraphed to Amsterdam: ‘Williams & Wilkins ten thousand subscriptions!’²⁶ In reality, however, this company never got more than a couple of thousand subscribers. Here, and almost everywhere else, people sat back and waited until the buyers turned up and then collected their share without having spent a penny on marketing. (The only exception was the firm Görlich in Milan, which managed to get more than a thousand subscribers in the Italian market, where hardly anybody read English.) So the sales contracts were not good. What they didn’t know in Amsterdam—Bart didn’t know this either—was that for science books there was often no more than 10% discount, and for some leading American journals, even less.

Von Eugen set out, together with Bart, to renegotiate the sales contracts. The first was that with Livingstone, in Edinburgh, because they thought that it would be highly lucrative to set up their own branch in the UK. The cost of renegotiating the contract was high because it contained no sales proviso and there was a penalty in case of cancellation, but the damage could quickly be made up as their office in London turned out to be successful. It fell to the lot of Peter Warren to end the contract with Williams & Wilkins in America. Warren (a noncombatant lieutenant colonel in the administration of the English army) was able to rent an office at the prestigious Academy of Medicine in New York, and from there he lobbied the National Institutes of Health in Bethesda and other medical establishments in the States. For here was plenty of

²⁶ Idem p. 2.

money to be had. When Bart left Excerpta Medica in 1962, he had accumulated 358,594 dollars from American project grants.²⁷

The reorganisation and the new international orientation of the company also claimed its victim. Erich Landsberger, nota bene the initiator, was dismissed in 1953.²⁸ Shortly after the appointment of the new young directors Van Tongeren and Warren, of whom he had not approved, Landsberger, in spite of rising debts, came up with some expensive plans to support the editing. It was clear that he would thus confront the owners rather than the board, considering their different interests, but no-one realised that the conflict would escalate and be settled only through arbitration, resulting in Landsberger's dismissal. This was due to a set up oversight in 1949. Alongside Excerpta Medica Ltd. (for profit), an Excerpta Medica Foundation (non-profit) was also set up for international recognition as a science documentation service and to be eligible for project grants. The idea was that the Foundation, of which Landsberger was director, did the work, and that Excerpta Medica Ltd. bore the costs, within the limits of an approved budget. But what if Excerpta Medica Ltd. rejected the budget of the Foundation? Such a conflict had not been foreseen in 1949. The double-faced Janus head of Excerpta Medica—Limited liability company on the one hand, Foundation on the other—is not of great importance in our history of science publishing. It served only financial purposes. This became abundantly clear in 1965, when the Foundation took over the capital of Excerpta Medica Ltd., which thenceforth became dormant. In 1971, when the company was sold to Elsevier, the Limited liability company had to be brought back to life, for who else was the owner?²⁹ But it was nevertheless strange that it was not the original shareholders, but the six managers of the Foundation who each received 1/6th of the purchase price...

But what *was* important were the Proceedings of congresses of medical specialists which the company produced from 1960 onwards, on the initiative of the editorial secretary.³⁰ These were original articles that

²⁷ Idem p. 7.

²⁸ Idem pp. 4–5.

²⁹ Letter from Bart van Tongeren to the author of July 24, 2004 (see also note 17); quote from p. 12A: 'In 1970, when the foundation was sold to Elsevier, fiscal advisers invented the *magic* argument that the appropriation of shares [by the foundation] in 1965 had only been *apparent* and that Excerpta Medica in fact had remained a limited liability company.'

³⁰ The unpublished 'Nonpareil' paper (note 25) pp. 5–6.

were accepted by the congress organisers, not articles that the editors of *Excerpta Medica* had approved. (The checking of such articles is usually marginal, and for this reason their status is *grey*.) The editorial board must have appreciated this initiative of James Cauverien, and his skill and creativity led to his being appointed co-director in 1962. As soon as he had demonstrated that he could publish *Proceedings*, and with speed, James thought up a variation. He offered the organisers of these congresses the possibility of putting all the articles at their disposal *before* the opening day, so that they could be distributed to the participants on the spot. It was a feat of strength to collect all the manuscripts in time, and to have them printed and bound, but their guaranteed sale at the congress by far exceeded the costs, especially if such a stunt could gratify the vanity of the organisers at the same time.

To publish *Proceedings* was highly remunerative, so other publishers had their eyes on these windfalls, too. A professor from Leiden, who had organised an international congress for physiologists and given the *Proceedings* to *Excerpta Medica* to publish, had the misfortune of having Robert Maxwell of Pergamon send furious telegrams to the main sponsors, the National Institutes of Health, with the accusation that American grants were being misused. Bart van Tongeren had signed the contract for *Excerpta Medica* with this professor, Jan Duyff. 'I knew him well,' he wrote in his memoirs, 'he was a man with plenty of backbone, even when up against a man like Maxwell. He'd start on the whisky at 11 o'clock in the morning and would expect me to keep him company; then we'd go for lunch in the restaurant *Oud Leiden*, where he had his own wine cellar, and such rounds of pleasure knocked me out until deep into the night.'³¹ The directors might have concerned themselves only with the business side of *Excerpta Medica*, but even so, glimpses into the medical world were by no means always denied to them. For instance, Martinus Woerdeman, chairman of the editorial board, introduced Bart to Peter Medawar, a celebrity who sat with him on the board. This was around 1960, the year that Medawar was awarded a Nobel Prize for his discovery of the rules for immunity in tissue transplantation. Bart's introduction into the medical world was going to play a role in the extension of the publishing list at North-Holland.

³¹ Idem p. 6.

This is the moment to mention briefly some of the developments that gave new content to the science of physiology in the 1960s. Traditionally physiology had consisted of a set of propositions on the functioning of human and animal bodies, established by means of experiment.³² Now, however, scientists became increasingly interested in unravelling the chemical structure of certain protein molecules (the immunoglobulins) that clarified the action of antibodies and opened the way to tissue transplantation. This knowledge stemmed from the 1940s. There were also new stimuli from the much improved knowledge of protein structures in general, whereby the foundations of molecular biology could be established, and the fantastic discovery of a double helix in the DNA-molecule that clarified the mechanism of the copying and transfer of genetic information. This knowledge stemmed from the 1950s, and the gains were largely due to refinements in X-ray diffraction, already an 'old' physical research method. From these three new basic sciences, immunology, molecular biology and genetics, emerged clinical research that had previously been unknown. It became possible to study the function of the hundreds of kinds of blood protein (haemoglobins), or that of membranes, muscles, nerves, or complete organs, whether or not in interaction with insulin, penicillin, enzymes, hormones... In physiology, too, this led to research into new therapies, for example, for leukaemia and cystic fibrosis.

Now, half a century later, we can see the molecular revolution that took place in biomedical science, but contemporaries did not fully realise this. Even those researchers closely involved found the idea of a revolution hard to grasp.³³ It was, *nota bene*, a publisher, Kurt Jacoby of Academic Press who pushed the Cambridge group of the English Medical Research Committee into setting up a *Journal of Molecular Biology*.³⁴ That was in 1959, six years after James Watson and Francis Crick

³² Olby & al. pp. 728–729, 740–741 (Chapter by John V. Pickstone).

³³ Krige & Pestre pp. 495–497 (Chapter by Pnina G. Abir-Am), where the word revolution is avoided, however: 'The molecular transformation of biology in the twentieth century, can be seen as an ongoing *progressive colonisation* by the so-called exact sciences: chemistry, physics, mathematics, and engineering, or rather combinations of their leading disciplines, such as organic chemistry and atomic physics.'

³⁴ Olby & al. p. 507 (Chapter by Robert Olby): 'It is true that the *Journal of Molecular Biology* did come into existence in 1959 but this was due neither to the initiative of the molecular biologists, nor to their determination to overcome resistance to their subject by existing journals. On the contrary, the idea of this journal originated from the founder-manager of Academic Press, Kurt Jacoby, who persuaded the scientists that his idea was worth supporting.'

there in Cambridge had discovered the double helix. Naturally Jacoby was keen to publish such a journal, and knew that the time was ripe to do so. At the time the less specialised journals, *Biochimica et Biophysica Acta* of Elsevier, *Biochemical Journal* and *Journal of Biological Chemistry* of the American societies published only sparsely on molecular processes. As we saw, it was only in 1962 that *Biochimica et Biophysica Acta* split off the section on nucleic acids into a separate journal, later to be called *Gene Structure*. But even if most of the science publishers understood little or nothing of these scientific fields, they must have realised that something important was going on, if only from the tremendous growth in governmental expenditure on biomedical research. In America they would have seen how in Bethesda, near Washington, one institute after another was established, each in combination with a clinic: the National Institutes of Health.³⁵ In the 1960s, when Van Tongeren was changing publishers, he knew that here were opportunities that he, as an enterprising publisher, could not afford to ignore. So it is very surprising that he had not yet even heard of *Biochimica et Biophysica Acta*.

Biological Publications

As we have said, in 1962 Bart joined the board of North-Holland. He had had enough of the mentality of Fred von Eugen & co, and Daan, for his part, wanted to be rid of marketing and sales. He was almost fifty and the weeks of travel these entailed became too much for him. It was quite by coincidence that they met. Daan reminisces:

As chairman of the publishing section of the Graphic Export Centre I flew to the book fair in Warsaw. I was sitting next to a woman, whom I vaguely knew, representing American publishers. No punishment, to be sure: vodka, champagne, liqueur. She took it all in her stride. I didn't, though. I wasn't quite myself when, later on that day, I went to take a quick look in that monstrous palace where the fair was to be held. In the Dutch section a man in his forties stood there talking into the telephone at the top of his voice. It went something like this:

Yes, I have an okay-ed KLM return ticket to Amsterdam, but you see, I always happen to change my booking for return flights... Sure, I do know that Brussels is not in Holland, but I want to spend the night in Brussels, and even if not, I never keep to my original schedule,

³⁵ Krige & Pestre p. 456 (Chapter by Christopher Lawrence).

for this I am known the world over... Sure, my wife knows, but why are you interested in the feelings of my wife? What is your name, and your age? ... No, sorry, I am busy tonight with my colleagues, but I will remember you next year... My name and affiliation? My name is VAN TONGEREN and I am a director of Excerpta Medica in Amsterdam... Whether I am a doctor? No, not a doctor but a *doctorandus*... What a *doctorandus* means? I will tell you next year, when we have dinner together... No, Excerpta Medica is not a capitalistic enterprise, we are a foundation and all members of the Board are social democrats as long as they have not saved sufficient capital to call themselves capitalists.

What a nutcase, I thought. Von Eugen was such an extreme figure that not a single science publisher in Amsterdam took Excerpta Medica very seriously, and this had influenced my idea of Van Tongeren. I had never seen him before. He looked fine. We got acquainted, and I liked him a lot.³⁶

After a few months Daan and Bart agreed on the terms by which Bart, as co-director of North-Holland, would take care of marketing and sales.³⁷ He asked for a share in the company, even if it were small, and it was agreed that he would acquire 10% of the share capital. Before this could take place it was necessary that a transaction between Daan and Gerrit de Vlugt, the first owner, be completed and the company provided with new capital. Bart had to wait until 1965 before he could buy a hundred shares at a nominal fee of a thousand guilders, shares which must have been worth many times that value on the stock exchange at the time.³⁸ In the meantime he had produced prospectuses and a catalogue list, and had also instigated discussions

³⁶ Letter from Daan Frank to Bart van Tongeren of 18 May 1975.

³⁷ Frank's unpublished papers *Memoir B* p. 3 in the section 'Van Tongeren, Wimmers, Krips & Baltzer': 'Already at the Frankfurt book fair of 1961 I thought about attracting a co-director for the marketing and sales. I talked about the idea with Jaap Noordhoff, with whom I had dinner, and he said: 'Did you ever think of Van Tongeren?' He had met the man shortly and his impression had been good.' In the interview on 12 February 2002, Bart said that he joined the North-Holland Publishing Company 'on the first of October 1962 at the invitation of its owner, Menkes Daniël (Daan) Frank, to become co-managing director, since the company had grown rapidly during the past ten years and Daan felt the danger of being overloaded.'

³⁸ Letter from Daan Frank to Bart van Tongeren of 22 October 1962: 'You will eventually participate for about 10% in the capital of North-Holland.' Agreement of 28 December 1965: 'Frank sells to Van Tongeren, and Van Tongeren buys from Frank, one hundred shares in the North-Holland Publishing Company of nominally one thousand guilders.' Remark at the end of Frank's *Memoir B*: 'We were grateful to the tax consultant, who took care that the nominal value of the shares was finally accepted as basis for the income- and company-tax.'

about the list and about investments, discussions that Wim Wimmers avoided, because he disliked ‘rows’, but which Daan had neglected for too long. Bart helped him to clarify the options for North-Holland, and to overcome his hesitancy, which so afflicted the cautious Daan. They became good friends and, for several years, spent a week travelling together to Berne or Paris, or whichever city happened to catch their imagination. They felt that they deserved it.³⁹ This way Bart didn’t have to wait long before confessing that sales gave him little pleasure. He preferred to contribute to developing the list, and saw sufficient means to employ a ‘real’ salesman.

In 1965 they appointed Johann Christoph (Chris) Baltzer.⁴⁰ He came from Blackwell in Oxford, where he had been sales manager and, thanks to his unprecedented capacity for work, had contributed considerably to their profits. Originally he was a Prussian from Königsberg. His father, a Nazi hater, had not survived the war, and after escaping to the West, Chris had found work in the bookselling business, first with Huber in Berne, then with Blackwell. Although highly educated and good at languages, as a German it was impossible for him to get promotion in this English family firm. Henry Schollick, a director of Blackwell, recommended Daan to him.⁴¹ North-Holland bought a house for Chris and his family, and after a few months he and the rest of his family could already speak quite a bit of Dutch. There was little else that he needed to learn; he knew perfectly well how a science book should be sold, and how to make a profit on it too. We read in Daan’s papers: ‘So in 1965 I had an excellent team at the top with Bart, Wim and Chris, and was extremely happy with the three of them.’ This sickly man in his fifties must have been thinking of his father, who died suddenly around this age, when he added: ‘Should I become seriously ill, the business will carry on.’⁴²

To return to Bart, he managed to convince Daan that the agreement that he had made with Piet Bergmans no longer worked to his advantage. We have already mentioned this gentlemen’s agreement whereby

³⁹ Interview of Bart van Tongeren on 12 February 2002.

⁴⁰ Frank’s unpublished papers *Memoir B* p. 4 in the section ‘Van Tongeren, Wimmers, Krips & Baltzer’.

⁴¹ *Idem* p. 4: ‘I saw Henry Schollick walking around at the Frankfurt book fair and asked him if he happened to know a young man who could be made responsible for the sales of North-Holland. ‘Everyone is asking me the same’ he said and walked on, but after five metres he turned and said: ‘I have one for you.’ That was Baltzer.’

⁴² *Idem* p. 4.

North-Holland would leave chemistry to Elsevier, and Elsevier would leave physics to North-Holland, as well as specialist areas of maths and economics, which we shall discuss later. However, the market for chemistry was still quite a lot larger than that of physics and was expanding at least as quickly. Bart argued that North-Holland had become strong enough to compete with Elsevier, and that Elsevier would find it difficult to penetrate into the territory of North-Holland.⁴³ Jacques Remarque, the biochemist who published *Biochimica et Biophysica Acta*, was forced to confirm this when he was sent out by Dolf van den Brink, director of Elsevier, to make contact with physicists. 'I tried, but I couldn't communicate with physicists. It really is quite another field.'⁴⁴ Bart, on the other hand, was able to get along excellently with biochemists, although he refrained from commenting on the content or the value of their field. 'Something that can work just as much to one's advantage as to one's disadvantage, and I didn't want to run the risk of having it work to my disadvantage.' He started close to home.

It must have been in the beginning of 1964 that he dropped in on Pierre Vinken, his friend 'around the corner', in the Van Boshuizenstraat in Buitenveldert (an Amsterdam suburb).⁴⁵ They knew one another from *Excerpta Medica*, where Pierre dealt with the neurological extracts. After Bart's departure he became director, without neglecting his neurological training, which he completed at the beginning of that year. He was then 36 years old. Bart, who dropped in on Pierre more often, now came with a question. Did he by any chance have any ideas about a book series in his field of study, or perhaps something else in the biomedical sciences? He asked, because *Progress in Optics*, a book series by Emil Wolf⁴⁶ was selling well, and a second book series, *Studies in Statistical Mechanics* to be edited by Jan de Boer and George Uhlenbeck,

⁴³ Interview of Bart van Tongeren on 7 March 2002. At that time also Elsevier took steps. Daan Frank writes (pp. 4–5 in the section '1955–1960' of *Memoir B*): 'I met Dolf van den Brink in 1963 (?) at a lunch of the Publishers Association, and there he did his utmost best to explain to me why the agreement had to be cancelled. Of course I couldn't object—I perfectly understood his position. But the curious thing was that the agreement was...continued. It was impossible for Elsevier to develop a physics list in the foreseeable future, and the same was true for North-Holland in the case of chemistry.'

⁴⁴ Interview of Jacques Remarque on 1 May 2003.

⁴⁵ Interview of Bart van Tongeren on 12 February 2002.

⁴⁶ See note 6 in this chapter.

was well-subscribed.⁴⁷ People now considered that new scientific insights were no longer to be found in monographs, for these quickly become out-of-date. What was needed were book series on specific subjects, to be open-ended and thus an endless source of income and profit for the publisher, if they were successful. Pierre did not take long to think it over and quickly came up with *his* idea.⁴⁸ Surely all those extracts that he had collected over the years, together with his friend, George Bruyn at *Excerpta Medica*, could form the core of a general work in this field? He invited Bart and George one sunny Saturday in March 1964 to his home in the Van Boshuizenstraat to discuss plans for a *Handbook of Clinical Neurology*. We quote from a letter written to Bart eleven years later:

Of all the publishers who we [Pierre Vinken and George Bruyn] approached at the time with the proposal for the publication of a handbook, you were the only one who didn't immediately look pessimistic, doubtful, fearful even. On the contrary, after a few general discussions on the matter you saw the possibilities of such a gigantic work, and you became the best seller of our idea.⁴⁹

Already on the following Sunday these general discussions led to plans for a handbook in 30 volumes of 650 pages (in the end it became 80 volumes), of which the text already largely existed (written by the

⁴⁷ The North-Holland series *Studies in Statistical Mechanics* started in 1962. The second editor, George Uhlenbeck, had discovered the electron spin, while working together in 1925 in Leiden with his colleague student Samuel Goudsmit—their experiments with a horizontal beam of silver atoms in a vertical magnetic field had shown that the atoms were differently deflected when their spins were parallel or anti-parallel with the field: the first observation of a purely quantum mechanical effect, and an early piece of evidence that the new quantum mechanics was both necessary and correct. George Uhlenbeck was from 1936 to 1939 professor of theoretical physics in Utrecht, before and after that episode at the University of Michigan in Ann Arbor, and since 1961 at the Rockefeller University in New York.

⁴⁸ Interview of Pierre Vinken on 1 October 2004: 'In the process of reading and excerpting thousands of neurological papers [for *Excerpta Medica*], I had come across excellent reviews. Wouldn't they make a useful book? So I got the idea of composing a handbook on neurology from the reviews I had collected together with my colleague George Bruyn, supplemented by reviews of the missing subjects, of course. At that time I was director of *Excerpta Medica*, but I didn't want to publish such a book myself. Was it perhaps something for North-Holland? Van Tongeren, whom I often saw, was initially sceptical, but after consulting Frank, who had to take the risk by investing a lot of money, he accepted the proposal by Bruyn and me, under the condition that we were able to bind top-neurologists to the project.'

⁴⁹ Letter from Pierre Vinken and George Bruyn to Bart van Tongeren of 21 May 1975.

“authors” of *Excerpta Medica*), and clever professors could undoubtedly be relied upon to provide any remaining summaries.⁵⁰ The sale of 700 copies seemed certain (in the end there were 3000). Just the thought of the fortune such a handbook would bring must have caused euphoria. But even so, what a risk!

Bart was certainly enthusiastic. This was something for the real entrepreneur. However, it was no simple matter to persuade Daan in this bold venture, even if only because Pierre Vinken and George Bruyn, the intended editors, had no name—or not yet.⁵¹ The least Daan could insist upon was that renowned neurologists endorse the desirability and viability of such a handbook. Naturally Pierre had thought of this, and lobbied one of his professors in Amsterdam.⁵² This Arie Biemond helped to get MacDonald Critchley (London) and Raymond Garcin (Paris) to support the plan, after which they were able to persuade five more neurologists to give their recommendations and contributions: Raymond Adams (Boston), Russell DeJong (Ann Arbor), Shigeo Okinaka (Tokyo), Sigvald Refsum (Oslo), and Klaus Zülch (Cologne). But for Daan it was highly questionable whether North-Holland could publish this work, because within the Dutch publishers’ organisation, there was a rule that directors did not publish one another’s work. The point was that Pierre had recently become director of *Excerpta Medica*. On Daan’s request Pierre explained the matter to the board of the Foundation of *Excerpta Medica*, but the board had no problem with this departure from the rule. Pierre, who we were still able to speak to and who couldn’t remember whether it was he or George Bruyn who first suggested the idea of the *Handbook of Clinical Neurology*, claimed that he could have published the work himself but that he didn’t wish to deprive Van Tongeren and Daan of the profits.⁵³ For him it was enough that he had his name to thank for it! Proudly he showed us the framed degree certificate, the doctorate *honoris causa* awarded to him

⁵⁰ Letter from Bart van Tongeren to the author of 24 July 2004 (see also note 22); quotes from p. 13: ‘The selection of hundreds of contributors was a gigantic task which Vinken and Bruyn executed with great knowledge and élan.’—further, in conjunction with the first sales to John Wiley and Igaku Shoin: ‘The price was already high, between 65 and 125 dollars per copy, depending on the number of pages of the volume.’

⁵¹ Interview of Bart van Tongeren on 12 February 2002.

⁵² Kenneth Ellison Davis, ‘The making of a handbook—An eyewitness account’, *Handbook of Clinical Neurology* 44 (1982) xiii. [Davis has *not* been eyewitness of the conception.]

⁵³ Interview of Pierre Vinken on 1 October 2004.

in 1981 by the Paris *Université Pierre et Marie Curie* for his editing of the handbook—a doctorate he had never strived for. These Parisians had failed to appreciate the enormous role of Bruyn in the editing... But in 1981 George had already long obtained his doctorate and was professor of neurology in Leiden.

From the above it can already be seen that the *Handbook* was a success. The first volume appeared in 1968, with contributions from 39 authors on *Headaches and Cranial Neuralgias*, and also in that same year a second volume, on *Diseases of the Basal Ganglia*, with 32 authors. In the medical press there was nothing but praise... ‘a valuable successor to the pre-war textbook by Oswald Bumke and Otfried Förster, with clear illustrations, bibliographies and indexes.’⁵⁴ Bart would be able to show off with it straightaway at the Frankfurt book fair. John Wiley & Sons from New York, for some time sole sellers in America of titles from North-Holland, promptly placed a standing order for a 1000 copies, which was just a beginning, and Igaku Shoin from Tokyo a standing order of 300.⁵⁵

In 1964, while they were still deliberating on this project, Bart went to visit Peter Medawar in London.⁵⁶ He also asked this old acquaintance from his time at *Excerpta Medica* for ideas on a book series in his own field. It was quite a step—for who approaches blithely a well-known scholar who has also written a book on *The Future of Man*?⁵⁷ He had purchased a bowler hat especially for the occasion so he could pay his call in the proper style, ‘with hat in hand’. Possibly Medawar was then already more interested in the relationship between genetic and cultural evolution than in the finesses of research into the chemical basis of heredity. In any case, he sent Bart to Albert Neuberger, also in London,

⁵⁴ Note 52 p. xvi.

⁵⁵ Letter from Bart van Tongeren to the author of 24 July 2004.

⁵⁶ Letter from Bart van Tongeren to the author of 24 July 2004 (see also note 22); quote from p. 14: ‘The visit to professor Peter Medawar at the institute for medical research in Mill Hill not only resulted in the series *Frontiers of Biology*, but after a number of years it led also to *Laboratory Techniques in Biochemistry and Molecular Biology*, a series edited by Thomas and Elizabeth Work of Medawar’s Mill Hill institute.’ In a letter of 9 May 1975 to Bart, Thomas wrote: ‘Without your persuasive personal intervention at the outset, neither George Popjack nor I would have agreed to start on the production of the *Laboratory Techniques* series. We did so with some misgivings and, as you know, Popjack had to drop out because of ill health before the project was under way. Fortunately my wife was willing to join me as co-editor, and we both have come to regard you as a personal friend. Over the years, many young biochemists have remarked to me on the value of the series to them personally.’

⁵⁷ Medawar *passim*.

who worked with new techniques and had made his name with a study of porphyrin synthesis in blood with the aid of radioactively labelled carbon.⁵⁸ Neuberger was still an enthusiastic researcher although in 1965 he was already 58 years old. As if he'd been waiting for such an opportunity all along, he immediately declared himself prepared to edit a series for North-Holland, which could be called *Frontiers of Biology*,⁵⁹ but he'd like also to have an American in on the project: Edward Tatum. Tatum had already been greatly talked of in the 1930s with his identification of the genetic precursors of the eye pigment of a fruit fly, to be followed by his spectacular discovery that sexual processes play a role even with bacteria.⁶⁰ Bart, who knew nothing about all this, only knew his name from the Nobel Prize he had been awarded in 1958, together with George Beadle and Joshua Lederberg. This was for the discovery that the growth effect of biotin—essential for all living organisms—is based on a code in the structure of this molecule. Neuberger & Tatum: that sounded perfect! Immediately Bart proceeded to New York, to prevail upon Tatum to collaborate on *Frontiers of Biology*.

How the author would have liked to have spoken to Tatum, there at the Rockefeller Institute on East River in New York! Even if only to hear his stories about the former laboratory for organic chemistry of our University in Utrecht, on the Catharijnesingel. He had worked here in 1936 and 1937, after taking his doctoral degree in Madison (Wisconsin), and saw how cleverly the discoverer of biotin, Fritz Kögl, analysed the structure of natural substances and synthesised reference substances for them. Moreover, here he had also met Nils Fries and discussed with him the growth effect in bacteria and fungi. This is all very relevant history.⁶¹ It would have been interesting, of course, just to hear about

⁵⁸ Fruton p. 380.

⁵⁹ Letter from Albert Neuberger to Bart van Tongeren of 12 April 1975: 'By coming at the right moment you deserve a large measure of credit for the success of *Frontiers of Biology*. But also your unusual ability as a publisher played a role, in combination with your great understanding of scientific and academic considerations. And then—your friendliness and the wide range of interests impressed me as much as your wisdom. Our relationship soon became one of personal friendship.'

⁶⁰ Fruton pp. 384, 433, 438, 443 (Tatum's words: 'The experiments imply the occurrence of a sexual process in the bacterium *Escherichia coli*').

⁶¹ Joshua Lederberg, 'Edward Lawrie Tatum', *Annual Review of Genetics* **13** (1979) 1–5.

his discussion with Bart van Tongeren, to hear what he thought of such a publisher, what he had to offer and what he suggested...⁶²

But sometimes one arrives too late, much too late. We read that Edward Tatum died in 1975, when he was 66 years old, after smoking a hundred thousand cigarettes, or perhaps many times more.⁶³ His photos show an almost completely bald man who looks out at us with a vague smile. Silently he tells us what he said at the end of his Nobel lecture, that modern civilization is full of dangers, and that we may well ask whether biological man is able to survive them, however versatile his genes may be.⁶⁴

We might even have joined Bart in his travels. This would not have been impossible. For we too were in New York in the 1960's, though for other reasons. We can still easily conjure up visions of those absurdly long queues before automatic rifle-toting customs officers at Kennedy airport, and those absurdly high skyscrapers silhouetted against the evening sky as the taxi drove to Manhattan through streets with Dutch names. And still we feel the way we felt on that first day, a lost soul making its way against the silent hastening throng, searching for the entrance to Penn Station, with an icy gale whistling down Seventh Avenue—where everything is too large, too vast, where all moderation, all delicacy, and the origin of all things are missing. We still baulk at the ghastly cliché of Europe in the materialistic culture of Affluence and Capital, for which we prefer to close our eyes as the train emerges from beneath the Hudson and speeds through the ruins of New Jersey.

⁶² Letter from Edward Tatum to Bart van Tongeren of 9 May 1975: 'My memory of our first meeting is still fresh. It was in my office at the then Rockefeller Institute, and you told me of your ideas for a continuing series *Frontiers of Biology*. I remember being impressed with the concept but not really sold in its practicality. However, not too much later you convinced me, with Albert Neuberger, to become general editors of the series. Little did I dream then that the series would be so successful scientifically, so beautifully produced, and, I judge, even successful economically!' This letter was written 6 months before Edward Tatum's death.

⁶³ Joshua Lederberg, 'Edward Lawrie Tatum', *Annual Review of Genetics* **13** (1979) 1–5.

⁶⁴ *Nobel Lectures Physiology or Medicine 1942–1962* (which, like all other Nobel Lectures up until the 1960s, were published by Willem Gaade, Elsevier) p. 610: 'It may confidently be hoped that with real understanding of the roles of heredity and environment, together with the consequent improvement in man's physical capacities and greater freedom from physical disease, will come an improvement in his approach to, and understanding of, sociological and economic problems.'

Naturally Tatum was delighted to take part in a book series with the title *Frontiers of Biology*, the chemist whose discoveries in biology were thanks to teamwork, and who knew how important it was to publish. When Bart asked him, he must have quickly come up with ideas, since the first edition appeared already in 1966. This issue of 110 pages consisted of an elaborated version of the lecture on microbiological models of cancer cells that the Russian Georgii Frantsevich Gause gave at the Rockefeller Institute in 1965. In the foreword to the series by the editors we read:

Molecular processes in genetics and metabolism exhibit patterns that are the same in all life forms, from bacteria to human beings. Because of these patterns the demarcations between various classical biological disciplines are fast becoming more blurred. At the same time, through specialisation in research techniques new boundaries are emerging, which urgently require a common conceptual framework. *Frontiers of Biology* attempts to provide this. . . . Most monographs in this series will consist of 200 to 300 pages, and it is the intention to publish them in quick succession. For this reason we are happy to seize this opportunity to lend our support in *Frontiers of Biology* by North-Holland Publishing Company.

Clearly the series was urgently needed, and therefore welcome, as can be seen from the 16 books that were published within three years. These included two exceptional works, on lysosomes and cytology, which took on the nature of a handbook and consisted of 1300 and 1500 pages.⁶⁵

Was it an easy success on the part of the publisher? This question arises as we come to Bart van Tongeren's third significant contribution to the list of North-Holland, *FEBS Letters*. The seeds for this were also sown in 1964, when Medawar informed him that a Federation of European Biochemical Societies had been established, the *FEBS*, and that it intended to set up its own biochemical journal.⁶⁶ This is why—and we

⁶⁵ Letter from Bart van Tongeren to the author of 24 July 2004 (see also note 17); quote from p. 14: 'Champion with 1500 pages was the *Handbook of Molecular Cytology*. The editor, professor Antonio Lima de Faria from the University in Lund, later expressed his gratitude for the full freedom he had had in the organisation of the book, and for the complete support I had given him in the production—as can be read in Antonio's letter to me of 12 May 1975.'

⁶⁶ Interview of Bart van Tongeren on 19 April 2005—mainly about the network of relations between Martinus Woerdeman, Jan Duyff, David de Wied and other medical specialists in the Netherlands, their connections with Albert Neuberger, Peter Medawar, Bill Whelan, Claude Liébecq, Prakash Datta and other specialists abroad, and the way Bart had used his access to the network since his years at Excerpta Medica; a day

see the bowler hat once again—he visited not only Albert Neuberger but also Prakash Datta, who also worked in London and had become treasurer of this same federation. Bart offered to publish the journal. But although he managed to gain Datta's trust (and subsequently that of other members of the board), he lost the project. Bart writes:

At the last moment the German science publisher Springer-Verlag came up with an offer that I couldn't beat. They offered to place their well-known *Biochemische Zeitschrift* together with its subscribers in a new *European Journal of Biochemistry*, on condition they could become the publishers. The board of *FEBS* seized the opportunity, and I lost it. And Claude Liébecq, would you believe it, this journal's great advocate and the one tipped to be editor-in-chief, had been in Amsterdam to discuss the set-up. Prakash, who would become a good friend of mine was there too, as well as the *FEBS* chairman, Bill Whelan. We read in the literature that I came over as being dynamic and made quite some impression on them.⁶⁷

We add here that up until the death of Ferdinand in 1965, Springer-Verlag would publish nothing in English. Ferdinand, who had made it into a renowned firm before the war and still had great influence, continued to believe that German could regain its old role as an important language of science. His influence was so great that all German publishers waited until the second half of the 1960s before publishing English books and journals.⁶⁸ In the meantime there was a lively market in the *lingua franca*. The merging of national scientific societies in Europe presented Springer-Verlag an opportunity to join the international arena once again as the societies sought to exchange their local journals for more concerted ventures. Despite the costs involved, they managed to carry off the *European Journal of Biochemistry* of the various biochemical societies as well as that of the astronomers, *Astronomy & Astrophysics*.

But North-Holland also saw opportunities in these mergers. We mention only the *European Journal of Pharmacology*, a journal that Bart managed to launch in 1967 with one volume per year, and which David de Wied, its energetic chief editor, was then quite quickly able to build

after this interview Bart sent the author documents on the founding of *FEBS Letters* and *European Journal of Pharmacology*.

⁶⁷ Letter from Bart van Tongeren to the author of 20 April 2005; the dynamics is depicted by W.J. (Bill) Whelan, 'The foundation and early years of FEBS', *FEBS Letters* **40** (1974) S154–S159, in particular S157.

⁶⁸ Götze pp. 74–87.

up to several volumes per year.⁶⁹ But to return to the *European Journal of Biochemistry*: this was potentially greater. Therefore, it is strange that the editor-in-chief of Elsevier's *Biochimica et Biophysica Acta*, Bill Slater, saw no threat in this new journal and even welcomed it. Biochemists, he wrote, are better served by a small number of large journals than a large number of small ones...⁷⁰ Elsevier, we hasten to add, was of course, less well served. And in hindsight there was one advantage to be had in the fact that North-Holland lost the project—not to mention the possibility of its being a success—for if the two great science publishers in Amsterdam had both published a major biochemistry journal, then that would certainly have posed a problem when they merged in 1970. However, Bart van Tongeren's dynamism set something further in motion for the *FEBS*. Again, in his own words:

In 1967 the *European Journal of Biochemistry* first appeared, this journal of Springer that was meant to counteract the biochemical journals from America. I saw it, and asked myself straightaway whether the *FEBS* couldn't supplement it with a letters journal, to counteract the *BBRC*, the American *Biochemical and Biophysical Research Communications*. I must have had in mind North-Holland's success with *Physics Letters*, and probably I also knew that Wimmers intended something similar that same year, in cooperation with two chemistry professors, [Jan Hoijtink from Amsterdam/Sheffield and Laurens Jansen from Geneva]: *Chemical Physics Letters*. So I put the idea to Prakash Datta, with whom I was still on good terms. It appears from an article by Bill Whelan that he too had a similar idea,⁷¹ so I don't want to claim it as mine alone. In any case, they asked North-Holland to submit a proposal for the publication of *FEBS Letters*, and they were keen on what I had to suggest. All letters (that is to say, short reports) would be assessed by a specialist, just as with *Physics Letters*, for only new, original work would be published. After the necessary adjustments and editing, the letters would be set in cold type

⁶⁹ When asked if it still was possible to start a useful journal in the medical field, Jan Duyff had referred Bart van Tongeren to the pharmacologist David de Wied in Utrecht (interview of 19 April 2005—see note 66). De Wied indeed saw possibilities for a new journal in his field, invited Jacobus van Rossum in Nijmegen to be co-editor, but cautiously opened the *European Journal of Pharmacology* with the following words: 'The prolific rate at which new journals are currently appearing and the growing confusion which has resulted have indeed been major factors in discouraging the Editors from assuming their present role, and are thus, paradoxically enough, the *raison d'être* of this preface.'

⁷⁰ E.C. (Bill) Slater, 'Significance of FEBS to world biochemistry', *FEBS Letters* **40** (1974) S164–S166, in particular S165.

⁷¹ W.J. (Bill) Whelan, 'The foundation and early years of FEBS', *FEBS Letters* **40** (1974) S154–S159.

by the unsurpassable Jan Krips and his staff, and take no longer than one month. None of the three other publishers who had been asked for a quote could match such a bid. We got the publication rights and Prakash Datta became editor-in-chief. The first issue of *FEBS Letters* came out in July 1968, and the hundredth volume in April 1979, and bear in mind that each volume was 360 pages. I read that it has become a large source of income for *FEBS*. I won't comment on what it meant for North-Holland.⁷²

Philosophical Diversions

All in all, the biochemical revolution gave a tremendous impulse to North-Holland, and to other firms also. And we note too that this revolution had been able to set the economic world in motion only indirectly, much less directly than the medical world in which it had given such a boost towards new diagnoses and new therapies. Perhaps it was the philosophical world that received the biggest shock. Inevitably notions about mankind and its origins were brought into focus. The episode of Bart's contribution to the North-Holland publishing list would not be fully covered if we did not do justice to this important aspect as well.

We shall confine ourselves to the ideas put forward by Jacques Monod in the 1960s. Monod, who was working at the Pasteur Institute in Paris, had been awarded a Nobel Prize in 1965 for his clarification of the mechanical manner in which genetic material is copied and proteins are synthesised. After receiving the prize he wrote a book, *Le Hasard et la Nécessité*, which was promptly translated into all the great languages. In it he gave new and strong arguments against the age old animistic (and vitalistic) interpretations of mankind, and against the idea that life—in particular that of human beings—served a purpose. For Monod, only those insights that had been given to him by molecular biology (the strict copying and transfer of proteins, which in rare but significant cases may still go wrong and then lead to a mutation) counted, because they provided the theory of evolution with an irrefutable foundation.⁷³ It is natural, he says, that when those who contemplate the long road that evolution has traversed in three billion years or so, the wonderful

⁷² Letter from Bart van Tongeren to the author of 20 April 2005; see also the article by Bill Whelan mentioned in the note above.

⁷³ Monod pp. 138–139.

richness of structures that it has produced, and the exceptionally purposive behaviour of living beings, from bacteria to humankind—they must doubt whether all this can have come about by some lottery under the supervision of natural selection, whereby in the final analysis the environment determines the winner from amongst the vast number of possibilities (mutations) that occur. But science allows us no other choice. The only problem here is that science does not place at our disposal an intuitively understood, unified concept of such a vast phenomenon as evolution. In this it is allied to abstractions in modern physics, which we also cannot visualise. But we know that this problem may not serve as an argument against a theory that is supported by logic and experiment. In the case of physics, whether it is about atoms or the cosmos, the problem is clear: the scale of the phenomena lies beyond our immediate experience. Only abstraction can address this deficiency, without resolving it. In the case of biology the deficiency is of a different order. The elementary phenomena on which everything depends, this mechanical copying and transferring, is relatively easy to understand. But this doesn't work in the case of an intuitive unified concept of living systems, of which the enormous complexity goes beyond any kind of understanding. But as with physics, neither can this psychological problem serve as an argument against the theory.

Monod concludes that the elementary mechanism of evolution is not only established in principle, but even that it is precisely known. And now let this mechanism account for the stability of species, by the constancy with which DNA is copied and in the directed activity, the purposefulness for which organisms are programmed! Such an account—doesn't this explain all the interest in biochemical research? Nor does it commit anyone to Monod's personal view on human beings—or call it his vision: that humankind has been awakened from its dogmatic slumber, and that the hard light of science now shows man that he is quite alone, 'at the frontiers of an alien world, a world that doesn't hear his music and is as indifferent to his hopes as to his sufferings or his crimes.'⁷⁴

In any case, it is at least a reasonable assumption that our life is an insignificant, fleeting phenomenon in the vast expanse of the cosmos. Let us just pause a moment at the thought, before going on to discuss the couple of astronomical publications that were added in the 1960s to North-Holland's publishing list.

⁷⁴ Monod p. 173.

In 1966, Albrecht Unsöld, director of the observatory in Kiel, published *Der neue Kosmos*. Like Monod, he may be regarded as an outsider, because North-Holland never published any of his work. In this work he makes clear that astronomy has become astrophysics—a science that deserves its place alongside the greatest discoveries of relativity theory, quantum theory and nuclear physics. Only physics can answer questions on the spatial structure of the world as a whole, the cosmos and its changes, its past and its future. And only physics will enable us to enter into space, unhindered by earth's atmosphere, to explore further. With new eyes we view a *new cosmos*; and as soon as we view the cosmos with new eyes, it is as if it changes. And Unsöld brings in a famous countryman: 'A century ago Alexander Humboldt came up with a design for a physical description of the world that he called *Kosmos*, and which served to make it clear to a wide circle of his contemporaries what physics research means for the development of mankind. Man must learn about this cosmos, and for him this was vital to forming the human mind.'⁷⁵

If this is so, and if the cultural significance of astrophysics is so great, then why did Daan Frank not allow Wim Wimmers to set up a journal? 'Books, yes; a journal, no. That was the message. And it was his money, so what do you do?' Wim said.⁷⁶ But this surprises us as Daan, the man of culture who so enjoyed listening to Sybren de Groot on philosophy, had long planned to publish the complete works of the great humanist Erasmus when the time was ripe. It surprises us, too, as he had allowed Bart van Tongeren to build up a list in contemporary biochemistry. Could the reason for his 'no' have been trivial? It can't be based on the financial risks entailed in Bart's grand plans in 1964.

Let us first discuss this as a possibility, for there is something to be said for it. In fact there was an interval of three years during which North-Holland permitted only a small number of publications in astrophysics. In 1967 Wim had published a collection in honour of Fritz Houtermans from Berne, a *Festschrift* that fitted in neatly with

⁷⁵ Unsöld p. vii.

⁷⁶ Interview of Wim Wimmers on 11 March 2003. Wim also said that the Leiden astronomer Henk van de Hulst had advised him in the early 1960s not to start new journals on astronomy, and that he regretted to have followed this advice; the astronomers in Western Europe then were striving for one common journal, and this was eventually acquired by Springer-Verlag.

a series of symposium reports on *Space Research* that he published in 1960. Houtermans had studied nuclear physical processes in the earth, the planets and the sun, and in 1929 made his sensational prediction that hydrogen is converted into helium in stars—directly, via proton-proton reactions, or indirectly via a cyclical reaction in the nuclei of carbon, nitrogen and oxygen.⁷⁷ Houtermans had told Wim at his *Fest* that he would have preferred a ‘roof’ rather than a book. This means a journal. We then have to wait until 1966 before Wim is able to publish the specialist *Earth and Planetary Science Letters* (in which a notice of Houtermans’ death had to be included), followed by the small journal *Physics of the Earth and Planetary Interiors*.⁷⁸ And aren’t we also reminded of the fact that in 1966 *Frontiers of Biology* started appearing, just when it was becoming clear that the large investment that was put into this risky project would pay off.

But there is another possibility for Daan’s ‘no’, one that is more complex, and can only be explained by taking into account the history of the development of *Astronomy & Astrophysics*. It has already been mentioned. In the 1960s, ever since the astronomical institutes in Europe had decided to set up a joint large observatory in the Southern Hemisphere, the possibility of a common journal was also discussed. However, it wasn’t until 1969 that they took the plunge and the journal was actually launched.⁷⁹ The French and German astronomers had found it hard to give up their national journals, not to mention the English—in the end they didn’t join in either. A politically neutral compromise was strived for whereby, in the end, Springer-Verlag (then in Heidelberg) was to publish it and the editing would be in Groningen. But in the meantime a Czech, Zdenek Kopal, had become tired of waiting. In 1965 this professor of astrophysics in Manchester began to set up something that the European institutes apparently could not. In that year the Americans Arno Penzias and Robert Wilson had

⁷⁷ J. Geist and H. Oeschger, ‘In memoriam F.G. Houtermans’, *Earth and Planetary Science Letters* **1** (1966) 137–138; see also Nye p. 520 (Chapter by Joann Eisberg): ‘Beginning 1929, Robert Atkinson, Fritz Houtermans, and Carl Friedrich von Weizsäcker investigated the proton-proton reaction and the CNO cycle, processes by which hydrogen nuclei might combine in the hot stellar interior to form helium and release energy.’ Hans Bethe proved ten years later that the energy, liberated in this way, agrees with the factual luminosity of stars, but (in spite of his Nobel Prize in 1967) he cannot be called the discoverer of nucleosynthesis.

⁷⁸ Interview of Wim Wimmers on 11 March 2003.

⁷⁹ Letter from Cornelis de Jager to the author of 13 June 2005; see also Götze p. 330.

established fairly easily that the cosmos is filled with a weak cold light.⁸⁰ This cosmic background radiation was a fantastic discovery, because it must be an after-glow of the Big Bang—a proof of the truth of a moment of creation. But it was not only this that set Kopal thinking: how come that it was only in America that people were receptive to this? He started to write letters and found four colleagues prepared to edit an international journal, and no less than sixty-five from Europe, America, Russia, Japan and Australia to form a board and who promised articles. He gave the first issue of ‘his’ *Astrophysics & Space Science*, which appeared in 1968, a high-pitched, provocative foreword.⁸¹ The journal came as a shock, also because of the controversial ideas that Kopal blithely published in it. Jan Oort, the astronomer from Leiden, who was making a case for *Astronomy & Astrophysics*, was angry because he saw his plans thwarted and blamed Cornelis de Jager, the astronomer from Utrecht who joined Kopal’s project and helped him find a publisher. This was a small firm, Reidel in Dordrecht, who had published his *Space Science Reviews* since 1962 to De Jager’s full satisfaction, and his *Solar Physics* since 1965.⁸²

Daan Frank knew all about it. In his posthumous papers we read that in 1898 Dirk Reidel had set up a printing firm, that Dirk’s son had been killed by a stray bullet in 1940, and that his grandson Anton carried on with the firm, together with his mother.⁸³ During the 1950s it became a substantial printing business, also for mathematical typesetting. Anton (described as ‘boorish and brash’ but also as ‘easy-going and optimistic’) managed to acquire substantial orders from both North-Holland and Elsevier, as well as from large publishing houses

⁸⁰ Kragh pp. 358–359; Nye pp. 532–537 (Chapter by Helge Kragh): [The discovery of the cosmic background radiation] ‘confirmed the theory of general relativity, which implies that the universe must have started in a space-time singularity. The big-bang scenario is not only compatible with the general relativity theory—it seems to follow from it.’

⁸¹ Zdenek Kopal, *Astrophysics and Space Science* **1** (1968) 3–5: ‘The pages of our journal are editorially open on equal terms to contributions from any country, and they can be written either in English, or French, or German or Russian (listed in alphabetical order). It is our firm belief that the costs of publication should be defrayed by the users (the subscribers), not by the authors, who are bound to be driven out of the scientific arena if their work is not sponsored by wealthy institutions or by governmental contracts. [...] The publication of our new journal represents basically an act of faith: namely, an unbounded confidence in the future growth of astrophysics.’

⁸² Cornelis de Jager, ‘Editorialities’, *Solar Physics* **196** (1996) 460–461.

⁸³ Daan Frank’s unpublished papers *Memoir B* p. 4 in the section ‘*The presson of printing*’ (‘De druk van het drukken’).

in England and America. In 1959 Piet Bergmans warned Daan that Anton was planning to compete with him, setting up his own publishing firm in the fields of physics and astrophysics, logic and econometrics. He printed books on these subjects for North-Holland, and must have thought—why don't I publish them myself? How Daan must have wanted to terminate his contracts with Anton! But he couldn't yet do without the rascal. 'Unpleasant discussions between Reidel and myself.'⁸⁴ We see just how serious Anton was from the books that he was already publishing in 1959, the first monographs on symbolic logic, epistemology, the sociology of science and related subjects, in a series called *Synthese Library* and edited under supervision of Jaakko Hintikka. But this didn't yet pose any real threat, as the market for these books was fragmented. This changed, however, when he went into physics and astrophysics. Cornelis de Jager, who we mentioned earlier, wrote: 'In 1961 he [Anton] came to visit me. He said that he was especially interested in the upcoming space research and asked me what he might publish in this field. Summaries, I told him, that's what everyone wants. After some discussion, he managed to persuade me to edit a new journal for him, called *Space Science Reviews*. The first issue came out in 1962, was well-received, attracted good authors and, in short, went well.'⁸⁵ Daan writes:

In 1962 Anton came to visit me. He had financial problems. He'd invested too much money in projects, good projects, but with a slow circulation rate. He asked me to contribute to the capital of Reidel with 51,000 guilders at par, and offered me two preferential shares, so that I would have a majority. I agreed. Besides this, I also gave him a bond loan of 75,000 guilders, for which he gave his machinery as security and I guaranteed printing assignments for 5 years. This made me president-commissioner of Reidel, and a bank manager from Dordrecht became joint commissioner. After thoroughly reorganising the firm with the help of accountants, Anton could go ahead once more. I must say, he did keep to his agreement; everything was done punctually: repayments, deliveries, financial statements. But I remained suspicious, and our relations together were always formal. He charged high prices. Did he charge hours worked for Reidel to North-Holland? I couldn't prove it.⁸⁶ [But we know from

⁸⁴ Idem p. 4: 'Ten years later I would have said "No more printing orders for Reidel", but I needed the man too much. So Reidel and I had a number of unpleasant conversations, after which I gave instructions to give Reidel as little to print as possible.'

⁸⁵ Letter from Cornelis de Jager to the author of 13 June 2005.

⁸⁶ *Memoir B* pp. 4–5: 'In 1969 Reidel got serious problems. Again he wanted money for further investments in buildings and machinery, but his Dordrecht banker and I

Hendrik Edelman, who worked at Reidel from 1965 until 1967 that he left because he couldn't bear to see Anton's tricks any longer.]"⁸⁷

We see from this history of the development of *Astrophysics & Space Science* that in 1968 Daan, perhaps to his own surprise, had actually become the owner of a large international astrophysics journal. Had he seen it coming? His 'yes' to Anton in 1962 may perhaps have prompted his 'no' to Wim in 1963.

If we exclude this Reidel affair, then during the 1960s Daan only devoted himself to the future of projects originating in the 1940s. His slightly fretful nature drove him each morning early to the office on the Keizersgracht—'a small figure in a large stately English car'—to go through the post with Bart and to let fly at an editor for his profligate use of pencils, but he was by now used to that.⁸⁸ He had more or less entrusted to Wim and Bart the core of the firm, the 'hard sciences' list that earned good money. But he kept for himself the list he started off with, those projects dating from the 1940s and with which he had wished to do his friends a favour: *Acta Psychologica* of Géza Révész and *Vigiliae Christianae* of Jan Hendrik Waszink and a few other scholars,

(I was his president commissioner) were as hard as nails against an increase of his credit and the issuing of more shares. Before matters were pushed to extremes, North-Holland was however taken over by Elsevier. Van den Brink wanted to take over my shares in Reidel's company as well, but I had the gentlemen's agreement with Reidel that he could buy these shares back before I sold my company. So it happened. Another bank was rather uncritically willing to give him credit. Incidentally—this transaction was very profitable for me. But a few years later Reidel had to sell his company to Kluwer. He was fired and left for America.'

⁸⁷ Interview of Hendrik Edelman on 14 March 2003: 'At the age of 21, in 1958, I started my career in library science. It started in The Hague, at Nijhoff's great export company of books and journals to the U.S. After a few years it became my duty to frequent all universities in the U.S., especially the new ones, to sell antiquarian journals and reference works. Then, in 1965, I wanted to become acquainted with science publishing and went to Reidel in Dordrecht. The main thing I learned there was that great profits could be made with off-prints. In general, the Dutch export of science journals was profitable, because of a tradition of type-setting, cheap labour and activities of the Dutch 'Graphic Export Center', but I couldn't stand the dishonest methods Reidel used to raise his profit. Already after two years, in 1967, I emigrated to the U.S. where I could become librarian at the Vanderbilt University in Nashville (Tennessee). In 1969 I moved to Ithaca (New York) to become deputy director of the library at the Cornell University, and finally, in 1978, to New Brunswick (New Jersey) to become director of the library of the Rutgers University, as well as professor of library- & information science.'

⁸⁸ Interview of Otto ter Haar on 14 May 2003.

where texts on early Christianity were dealt with.⁸⁹ He felt he should continue with these journals, although they earned him little money, because he valued having a name in the humanities. And it gave rise to an idea. Waszink had suggested to him in 1947 that he write to the Academy to remind them that during the war a re-publication of the writings of Erasmus had been discussed. There had also been such plans in Germany. Just imagine if he could publish the *Opera Omnia*! Their answer was: ‘...most interesting, and if the times had been different, we would have willingly cooperated, but...’⁹⁰

In those early years there was another acquaintance, Hendrik Pos, who had asked him to publish the Proceedings of an international philosophy congress that was held in Amsterdam in 1948. He edited them together with his younger colleague at the University of Amsterdam, the logician Evert Beth, who at that time, at the age of 40, was already famous for his pioneering work on the foundation of the exact sciences. Karl Popper, not yet famous, had contributed an article on the meaning of predictions in the social sciences. Daan had not only published this congress, but also the following one in Brussels. But with the 14 volumes of the latter he had bitten off more than he could chew. Just as with the publications of Révész and Waszink, he had been driven by a personal interest. Flattered by Beth’s promise to edit a series of books on logic, together with the intuitionist mathematician Luitzen Brouwer and his successor Arend Heyting, all from Amsterdam, in 1949 he had put his *Studies in Logic* onto the market. Beth and Heyting, as well as several mathematicians from elsewhere, such as Haskell Curry, Imre Lakatos, Abraham Robinson and Patrick Suppes, all published profound works in it.⁹¹

⁸⁹ Daan Frank’s unpublished papers *Fragments* pp. 47, 50.

⁹⁰ In a conversation on 28 June 2005, Anneke Frank-van Westrienen showed the diary she held on the early years of her marriage with Daan shortly after the war, and pointed at the following lines: ‘*Aan zijn tafel schreef hij vele vellen papier vol...*’ He filled whole sheets of paper with complicated calculations and shook his head, disheartened. What he had in mind was something great—too great for him alone: he wanted people to read again in the writings of a reasonable and learned man who centuries ago had preached tolerance, writings of eternal value. He approached the Academy [we add: on 25 November 1947, 17 February 1948 and 17 March 1948] and travelled around in various countries in search for help, but everyone said: very interesting, we would love to help you, and if the times would have been different...’

⁹¹ Daan Frank’s unpublished papers *Memoir B* p. 1 in the section ‘1955–1960’; Frank writes here to have spoken at a congress with the name ‘Logic Colloquium 76’ about the genesis of *Studies in Logic* and about the role Beth, Brouwer and Heyting had played,

During the course of the 1960s this series grew to sixty titles, but each had no more than a couple of hundred buyers at the most. Only the thick *Metamathematics* by Stephen Kleene sold several thousand copies. To strengthen his ties with this successful author, he went to visit him in 1967, in a snow-swept Madison with icicles hanging from his hotel window, and where he discovered on what friendly terms a professor could be with his former students, ‘long before there was any question of democratisation back at home.’⁹² The same trip also took him to Alfred Tarski, the ‘grand old man’ whose *Ordinal Algebras* he had published in 1956, and he had more work of his in hand. ‘Tarski was certainly the most famous logician, but also the most difficult, and he made impossible demands where typography and royalties were concerned’, writes Daan: ‘Even so, he could be very kind, as well. We were walking across the campus at Berkeley, when the campanile clocks began to chime. “Do you hear that?” said Tarski, “they’re doing that for you and for your *Studies*.”’⁹³ This is apt. We still hear from mathematicians that, for a great part, the exceptional quality of this original series is also thanks to Daan’s achievement, with all the personal attention that he gave to his authors. Tarski came originally from Warsaw, where in the 1930s he wrote his pioneering article on the concept of truth in formal languages, and where he was succeeded by Andrzej Mostowski, a student. This young Polish man had joined the editing team of Daan’s *Studies in Logic*, and Daan quickly formed a strong friendship with him. On Andrzej’s telegraphed request, Daan had immediately sent him penicillin, by air, to save the life of a son who was seriously ill. ‘When I was in Warsaw I used to visit him in his small four-roomed flat, and each time I was amazed at how he was able live there with his wife, mother-in-law and children.’⁹⁴ Mostowski, in turn, had Einar Fredriksson as a student, a Swede whose doctorate on *Algebraic relational models of some calculi weaker than predicate logic* he supervised. In 1969, when Daan

but we have not been able to find and study his speech. This is much to be regretted, in view of the great importance of the book series.

⁹² Idem p. 2.

⁹³ Daan Frank’s unpublished papers *Memoir B* p. 2 in the section ‘1955–1960’.

⁹⁴ Idem pp. 1–2: ‘Mostowski died already young, in his early sixties. Around 1955 he sent a telegram and begged me to provide him as soon as possible with penicillin for his sick son. I sent it by air freight and it saved his son’s life.’

asked Andrzej for a student who could help with his ‘soft’ list, he was offered Einar on a silver tray.⁹⁵

In this case it was not just *Studies in Logic*, but also *Contributions to Economic Analysis*, another of Daan’s ‘hobbies’. He started on this series in 1952 to oblige Jan Tinbergen, not because he expected to get much from it. What publisher wouldn’t take his chance with such an internationally renowned man? Tinbergen had signed up to edit *Contributions*, together with his colleagues Willem Witteveen and Louis Zimmerman, and promptly accepted a work from the latter on the trend to monopoly formation. Jan Tinbergen had actually started to work together with Daan Frank in 1943 when he had his book *Economische Bewegingsleer* (*The Dynamics of Business Cycles*) published by North-Holland, and it was Daan who had charge of it. There were three reprints. They were on first name terms, but otherwise remained formal: ‘He wasn’t someone you asked out to lunch.’⁹⁶ Understandably—apart from the fact that Tinbergen was an ascetic—for this professor, who was also director of the Central Planning Office for economic recovery in the Netherlands, just didn’t have the time, busy as he was with the development of the dynamic economic models for which he and Ragnar Frisch would be awarded the Nobel Prize in 1969. Three years after the prize, and twenty years after the first issue, he remembers how *Contributions* started out:

⁹⁵ Interview of Einar Fredriksson on 5 November 2003. Fredriksson is in many respects a remarkable man. He was born in 1942 in Lund and studied mathematics and philosophy in Warsaw. Already 6 years before his graduation he started to work for Daan Frank, who was befriended with his thesis adviser and had asked the latter for young men that wanted to develop North-Holland’s journal branch for economy, logic and mathematics. Joon Stomp, who had set up this branch, was leaving in 1969 and Daan had a vacancy. Einar accepted the challenge and came to Amsterdam. The series of monographs on economy, started in 1953 and edited by Jan Tinbergen (after 1969: Dale Jorgenson), became the carrier of 35 to 40 journals, in which also Bart van Tongeren and Ellen van Kooten became involved and where until today the world literature on econometrics is being published. ‘It was my own idea,’ he said, ‘to start in 1977 a series of handbooks, which now [in 2003] counts 21 volumes and employs thousands of authors. Other publications I had to take care of were in the field of logic, but they were not so numerous, since the market is very specialist. They started already in 1949 with *Studies in Logic*. Before I came, North-Holland did little in the field of mathematics. It was Springer’s territory, with famous editions on pure mathematics, but I ventured in journals on applied mathematics and computing, with titles like *Artificial Intelligence*, *Mathematical Programming* and *Theoretical Computer Science*. When I left the company in 1987 and started a company of my own, I had launched about 40 journals on mathematics.’ We add that the tremendous growth of computer science, with its organisations, special interest groups and specialised journals, is described in Nye pp. 598–614.

⁹⁶ Daan Frank’s unpublished papers *Memoir B* p. 3 in the section ‘1955–1960’.

Once when we [at the Netherlands Highschool for Economics (NEH) in Rotterdam] were faced with a manuscript that ‘was too big for a napkin, but too small for a tablecloth’—I mean too big for an article in a journal and too small for a book—Mr Frank helped us by setting up the series *Contributions to Economic Analysis*, which would accommodate such pieces of writing. This series became a great success. While Frank, in the beginning, rather too generously, included a number of dissertations in this series, later on we decided between ourselves to raise the standard, and at the moment it is a matter of status to be included in this series, much coveted by many a foreigner. In particular it opened the American market for us.⁹⁷

The series grew from 20 titles in 1960 to 62 in 1969 when Dale Jorgenson from Berkeley took over the editing. Tinbergen published in it himself, including his *Economic Policy—Principles and Design*, and the great followed: Michael Michaely, William Branson, Ray Fair, Lawrence Klein...almost all of them Americans, and the latter also a Nobel laureate...All those Nobel Prizes! And once again it was Jan Tinbergen who advised setting up more economics and econometrics journals, and immediately assigning subjects and possible editors to them. He no longer directed this advice to Daan Frank, but to Bart van Tongeren who started up a similar journal list with his colleague Ellen van Kooten, a list that Dale Jorgenson and Einar Fredriksson would then enlarge. The Swedish philosopher, who in 1969 had arrived in Amsterdam from the editing office in Warsaw, turned out to be highly motivated in helping the economist from California with acquisition—an acquisition that would lead to the forty journals of North-Holland in which the world literature on econometric is to be found.⁹⁸

Let us move on to something else. It comes as no surprise that the first *Contributions to Economic Analysis* should come from Rotterdam, the biggest trading city of the Netherlands with the largest harbour in the world and where the Netherlands Highschool of Economics (now the Erasmus University) found its natural place. What does surprise us is that this same city should take the initiative to republish the *Opera Omnia Desiderii Erasmi Roterodami*.

⁹⁷ Quote from Tinbergen’s speech at the retirement of Frank, in: ‘Contemplations of the future at a farewell’ [*Toekomstbespiegelingen bij een afscheid*], unpublished brochure (1972).

⁹⁸ Interview of Einar Fredriksson on 5 November 2003.

In April 1960 Cornelis Reedijk, librarian, wrote a letter to the mayor.⁹⁹ His dissertation on Erasmus' poetry has given him valuable knowledge of the unique collection of Erasmus manuscripts in the city library. Now that the city centre, which was destroyed during the war, has been largely rebuilt, and restoration of the Laurens Church has brought back its medieval past, he requests this Most Reverend gentleman to think once more about Erasmus. Wilhelm Lichtenauer, the energetic secretary of the Chamber of Commerce as well as an amateur historian and chairman of the historical association *Rotterdamum*, supports him. What has the city ever done to commemorate its great and learned son, apart from a by now age-old statue? The moment is ripe. Amazingly, a priest, Regnerus Post, finds the document that establishes the man's year of birth near Rotterdam. Nobody had known it before: 1469. In 1969 it will be five hundred years ago. An opportunity, therefore. But what can the irenic Erasmus mean to a commercial city? How contemporary are the inspired labours that he performed his whole life long? His writings, the *Opera Omnia* published in Basel by Rhenanus shortly after his death, and again by Clericus two centuries later in Leiden, are both as old as the hills and gathering dust. It is a bitter pill that it took a German to remark upon the fact, of all times at the height of the plundering of Europe by the Nazis, in 1942. He, *Herr* Karl Meissinger in Munich would set a group of scholars to work *unter straffer Oberleitung* to republish the *Opera Omnia*—a plan, of course, that came to nothing, not least because the *Oberleiter* Otto Schottenloher soon died...

An act of reflection in a working city as Gerard van Walsum, the mayor, ponders the matter. The idea of a commemoration in 1969 appeals to him. He is in need of advice, and goes to the Royal Academy to ask whether the *Opera Omnia* should be republished and to whom this should be entrusted.¹⁰⁰ After a year has passed he hears from Jan Bakhuizen van den Brink, chairman of the committee to which the request has been passed on, that a new publication with scholarly introductions and explanations is certainly desirable, but that this can only be achieved if a body of scholars from many countries works on

⁹⁹ Cornelis Reedijk, 'Tandem Bona Causa Triumphat—Zur Geschichte des Gesamtwerkes des Erasmus von Rotterdam', *Vorträge der Aeneas-Sylvius-Stiftung an der Universität Basel XVI* (1980) pp. 44–47.

¹⁰⁰ Letter from Cornelis Reedijk to Gerard Ewout van Walsum of 11 February 1964.

it together—which will obviously cost a great deal of time. But already foreign academies have been approached for the task, and in December 1963 the mayor is able to receive a dozen scholars in his city hall, the *Conseil international pour l'édition des oeuvres complètes d'Erasme*, namely Jan Bakhuizen van den Brink, Léon Halkin, Otto Herding, Kazimierz Kumaniecki, Pierre Mesnard, Roger Mynors, Regnerus Post, Cornelis Reedijk, Franz Schalk, Antonio Villanova, Christoph Vischer and Jan Hendrik Waszink.¹⁰¹ The plan appears in the newspapers.

Until that moment Daan knows nothing of all this. Waszink, for whom for years he has published the journal *Vigiliae Christianae* and now turns out to be in the *Conseil*, has told him nothing. Daan phones him straightaway. Waszink can surely tell him to which bank account he can transfer 5000 guilders so that editing can start immediately?¹⁰² Expedient, that money: a claim. He's been waiting for this for years.¹⁰³ He doesn't know it, but others are also making their approaches: the Universitaire Pers in Rotterdam, Mouton in The Hague, Fromman in Stuttgart, ...but not Brill! Surely an Academy publication would be entrusted to none other than the Academy publisher? A few months later, in March 1964, Daan speaks to Waszink and Bakhuizen van den Brink and it is agreed that North-Holland may make the first offer for the publication, according to article 1 of the publishing contract.¹⁰⁴ Not only this, Daan also presents them with the bait of 200,000 guilders for the payment of editors. Even so, he should understand that this concerns not just the Academy. This is truly an international venture. Professor Bakhuizen van den Brink knows that promises have been made to a German publisher...but if Mr Frank would consider talking to Mr Holzboog of Fromman about the possibility of working together...Daan

¹⁰¹ *Opera Omnia Desiderii Erasmi Roterodami*—pp. xv–xxi of the General Introduction of 17 March 1969 written by the editorial board at that time (Jan Hendrik Waszink, Léon Ernest Halkin, Cornelis Reedijk and Cornelis Bruhl).

¹⁰² The fact is mentioned in the letter of Daan Frank to Cornelis Reedijk of 26 Februari 1964; Reedijk thanks Frank on 18 March 1964 for the 5000 guilders: 'a firm material foundation' for the ideal he had since his dissertation of 1956 on Erasmus' poetry, namely the ideal of a new edition of all Erasmus' works.

¹⁰³ See note 90 of this chapter.

¹⁰⁴ The letters by Daan Frank to the Academy of Science about the new Erasmus edition are dated 5 and 11 March 1964 and are addressed to Jan Nicolaas (for his proximus and only for his proximus 'Nico') Bakhuizen van den Brink in Leiden, whereas the latter answered on 10 and 14 March 1964; the 200 thousand guilders are offered in the letter of 5 March, and the competing publisher Günther Holzboog is for the first time named in the letter of 14 March.

swallows hard, agrees ('no fundamental objection') and, although he has never heard of the man manages to track down *Herr* Günther Holzboog in Stuttgart and in June to have 'a most pleasant discussion' with him about a contract on a 50:50 basis, until Günther realises that he, Daan, has already committed himself to 200,000 guilders for editing honoraria.¹⁰⁵ The German has nowhere near that amount of money for his half and will have to apply to the *Deutsche Forschungsgemeinschaft* for it. In July he decides not to go ahead with the contract. Daan can easily raise the money. *Nuclear Physics*, his constant source, enables him to publish what he has always wanted: 'the Erasmus'.

And the risk? Hard for him to judge; the natural science books he is accustomed to quickly lose their value—something he knows only too well—but the humanities books that keep their value have to be kept in stock for years to come, which is expensive—just how much so, that's something they can tell him at Brill. Actually it doesn't greatly matter to him if he never gets back all the capital that he is considering putting into the *Opera Omnia*, although he is careful.¹⁰⁶ In the contract that North-Holland signs with the Academy in April 1965, he stipulates precisely the editorial and production procedures including the deadlines when the editorial secretary (who will be Cornelis Bruchl) will deliver the manuscript copy, and commits himself to 30 volumes of at least 300 pages per volume with an edition of 250 copies, for which the purchasers must be guaranteed, with the hope that they could double this number over the years. He chooses the paper personally, and has proofs made until he is satisfied with the page lay out: Erasmus' Latin in solid lettering at the top, and in small lettering the notes with comments and references in German, English or French allowed to the editors in two columns underneath. The first text, he already knows exactly, is to be Erasmus' work from his youth, titled *Antibarbari*, with Kazimierz Kumaniecki as editor. Appropriate? The work is an ironic commentary on Medieval scholars—'barbarians' who refer only to one another and disregard works from Antiquity, for in pre-Christian times nothing profound could possibly have been thought of and written

¹⁰⁵ The above 'Academy letters' are continued by one of 9 May 1964 from Cornelis Reedijk to Daan Frank (on editorial and financial matters), and by three in the other direction, namely of 10 June (on the collaboration with Günther Holzboog), 20 July (on the withdrawal of the latter) and 17 August 1964 (on the contract of the Academy of Science with North-Holland).

¹⁰⁶ Interview of Anneke Frank-van Westrienen on 28 June 2005 (see also note 90).

down.¹⁰⁷ Appropriate, therefore, insofar as modern science is imprisoned in its presuppositions, and blind to its roots.

In the meantime Daan had hoped to be able to take over Brill. Brill, renowned worldwide in classical publications *Tuta sub aegide Pallas*, was established in 1848 by printer Evert Jan Brill in Leiden, so that he could continue the bookshop and publishing firm established by Jordaan Luchtmans in 1683 under his own name.¹⁰⁸ This venerable firm had remained silent when 'The Erasmus' was under discussion. Under the direction of F.C. Wieder Jr it is languishing.¹⁰⁹ Ernst van der Beugel who, after being a minister, has become director of the national airline company and is also president-commissioner of Brill, is worried about the future of the firm. There is too little enterprise. For him the limit is reached when it becomes known in 1966 that not Brill, but North-Holland is to publish the works of Erasmus, and he comes to the conclusion that Brill should merge with North-Holland.¹¹⁰ Van Tongeren met Wieder quite by chance and made a tentative suggestion to this effect, after which Wieder is unable to get out of arranging a meeting with his president-commissioner. Naturally Daan Frank is there too. He meets F.C. Wieder for the first time, discovers they are the same age but not Wieder's first name: is it Frans or Floris, or Frits? He finds him old-fashioned and wonders whether this is the man with whom he could revive this stagnating firm. Pretty soon they are no longer talking about working together, but about a takeover in cash. Daan is thinking of an issue on the stock exchange, via Brill, for which he will have to

¹⁰⁷ Characteristic by Hans Trapman, nowadays (2007) secretary to the editorial board of the *Opera Omnia*; Trapman has been helpful in providing us with copies of the letters mentioned in notes 100, 102, 104, and 105; he wrote about the ongoing history of this enterprise under the title: 'Een werk van lange adem' ('A long-winded work') in *Radar* **96** (1995) 326–341.

¹⁰⁸ Ophuijsen *passim*.

¹⁰⁹ Daan Frank's unpublished papers *Memoir B* p. 1 in the section 'E.J. Brill': 'Wieder had the name of being a nice and honest man, but we didn't know him well since he seldom turned up at the meetings of the KNUB, our publishers alliance, and although the company's shares were in the hands of descendants of the Brill family, we knew that Brill wasn't doing well—the shares were namely registered on the unofficial stock market.'

¹¹⁰ Idem p. 2: 'Ernst van der Beugel, president commissioner of Brill, invited Bart and me [on 16 February 1966] at his home in Leiden in order to discuss the possibility of a take over. He then went on and asked professor Jaap Kymmell, at the time partner of [the bankers] Pierson, Heldring & Pierson, to elaborate on the terms under which Brill could be taken over by North-Holland. These terms were initially acceptable, both for Brill's board of commissioners (Van der Beugel) and for Wieder, Bart and me. Later on, Wieder got cold feet.'

sell a part of his shares in North-Holland.¹¹¹ ‘Frans’ doesn’t dare say ‘no’ to his face, but feels threatened. A takeover proposal is formulated, which Van der Beugel puts before the board in April 1966, and once again in December. The president-commissioner threatens to resign if the director, who is dragging his feet, rejects the proposal. Wieder says he will think about it again, and in January says ‘no’. Van der Beugel doesn’t resign.¹¹² Two days earlier the director had set forth to Amsterdam to tell Daan personally ‘that he had thought it over again, that he was afraid of losing his independence, and that he, as director of Brill, would hand in his notice if the takeover went ahead.’¹¹³ The takeover never happened. For Daan this was a personal disappointment, but for the further development of North-Holland it was not, in our opinion, a disaster.

We have come to the end. On 27 October 1969, in the now completely restored Laurens Church in Rotterdam, after glowing words from Professor Samuel Dresden from Leiden on the enduring significance of Erasmus for our civilization, the moment has arrived to hand over the first copy of the *Opera Omnia Desiderii Erasmi Roterodami* to Juliana, Queen of the Netherlands—a showpiece. Amongst the many hundreds of dignitaries invited to the occasion we see Menkes Daniël Frank, the publisher. He sits in front; it is his finest hour.¹¹⁴

¹¹¹ Idem p. 2: ‘My plan was to become delegated commissioner and reorganise the company, together with Wieder, then sell part of my own North-Holland shares on the stock market and buy with the money the Brill shares I would issue on that market.’

¹¹² One may read the Minutes of the meetings of Brill’s commissioners of 22 April and 2 December 1966, and of 13 January 1967; the second meeting is on Wieder’s threat to resign as director if Brill is taken over by North-Holland, the third on the acceptance of Wieder’s wish to stay independent.

¹¹³ Daan Frank’s unpublished papers *Memoir B* p. 2 in the section ‘E.J. Brill’: ‘He [Wieder] argued that we [North-Holland] had no man who could manage his very special, difficult list. His resistance to the take over by North-Holland was psychological. This man had already my age, had only recently become director of the company, after years of waiting as the second in command, and may have been afraid for the innovations I had in mind.’

¹¹⁴ The *Nieuwe Rotterdamse Courant* published on 19 April 1969 two page-long articles on Erasmus, but nothing on the commemoration of 27 October (Erasmus’ birthday) in the Laurenskerk that was organised by the city council. The *National Zeitung* from Basel did (Basel being the town where Erasmus died) and wrote about the proud publisher of scholars... *Der Gelehrten-drucker Menkes Daniel Frank aus Amsterdam geht dem glänzendsten Ereignis seines Lebens entgegen...* At this occasion also the *Praemia Erasmiana* (Erasmus Prizes) were handed to the philosophers Gabriel Marcel and Carl Friedrich von Weizsäcker; the latter is mentioned in note 77, and there as physicist.

CHAPTER SEVEN

THE ASSOCIATED SCIENTIFIC PUBLISHERS

Did it come as a surprise? A month before, in September 1969, North-Holland and Elsevier (to be exact: Elsevier Science Publishing) had announced that they wished to merge. In their joint communiqué they referred to themselves as equal partners,¹ as illustrated by the figures: North-Holland published around 50 titles per year, Elsevier 75; and since the war these totalled 600 and 900, respectively. But books were not their most important publications. These were science journals: North-Holland had 34; Elsevier 54. Anyone who might therefore think that the latter was somewhat larger should know that in that year North-Holland's *Nuclear Physics* ran to 18,000 pages, while Elsevier's *Biochimica et Biophysica Acta* would not exceed 15,000. According to the communiqué: 'A better understanding of the size of these journals can be obtained by comparing them to an encyclopaedia: one yearly publication contains more pages than the complete *Winkler Prins* of 20 volumes. In their fields they belong to the most important in the world. Two to three times a week both publishers send their journals by air to New York, so that just a few days after publication they can be found on the reading tables of the universities, hospitals and other institutes for scientific research in the United States and Canada.' The communiqué also indicates briefly the reason for this amalgamation: by joining forces the publishers would be 'better equipped to cope with future demands in the transfer of scientific knowledge.' This last is plausible, for collaboration leads sometimes—not always—to lower costs and a better product. But this was not the true reason.

Daan wanted to sell his publishing house; this was the real reason.² He had wanted to do so for the previous year, since his 55th birthday.

¹ The *Communiqué* of one and a half page is dated 4 September 1969, (probably) composed by Bart van Tongeren and issued by the North-Holland Publishing Company—Amsterdam.

² Letter from Bart van Tongeren to the author of 24 July 2004, p. 16: 'Unfortunately it was Frank's health that made him decide to withdraw from the busy and often pressing business life, and to cash his shares of North-Holland.'

Struggling with his diabetes, he felt increasingly overworked. Then his doctor would send him to Beaulieu, close to Nice, to contemplate the rippling waves from behind the bougainvillea.³ So, to be able to retire from business life, he went in search of a firm that would take over North-Holland. We recall that he had in his possession all the North-Holland shares, apart from the 10% that Bart van Tongeren had been able to take over from him a couple of years before. All this could remain in the family if his son wanted to carry on the firm. But 21-year-old Marius had other ideas. His choice for economics seemed appropriate, and although he may have stood in awe of his highly successful father, he did not relish the thought of going through life as ‘the son of’.⁴ Theoretically the shares could have been sold on the stock market, but that would have been unwise, for it was impossible to estimate the value of this relatively small, specialist firm. He had to look for a buyer who was familiar with science publishing. Everyone who knew him and saw him struggling understood what he had in mind.

Piet Bergmans, the friend with whom he had made his ‘gentleman’s agreement’ must also have understood, and spoken with Dolf van den Brink about an eventual takeover by Elsevier; otherwise we can’t explain why Dolf, when he saw Daan and Bart at the Frankfurt book fair in 1968, immediately raised the matter with them. But he struck the wrong tone. According to Bart, Dolf explained at length how a publishing house ought to be financed, and how much North-Holland might gain from a professional management should the board of Elsevier be prepared to accept them into the holding.⁵ Elsevier, which had plenty

³ Interview of Anneke Frank-van Westrienen on 3 August 2006; her husband had also had thoughts of a sudden death, she said, a death such as his father Emanuel befell while he still was young, and in order to prevent a stroke or a cardiac crisis his doctor had advised him to deliberately reduce his work load by going from time to time to Beaulieu, for ‘a while’, alone.

⁴ Letter of Marius Frank to the author of 14 July 2006; almost forty years after his decision to steer his own course, Marius writes: ‘I was right. I have seen too many who became unhappy by not going their own way or who felt obliged to stay in the family company. This doesn’t mean that publishing had anything appalling for me—on the contrary I found it fascinating.’

⁵ Letter from Otto ter Haar to the author of 26 January 2006 (*not* from Bart van Tongeren, who said not to remember the details): ‘Piet Bergmans and Daan Frank had already spoken at length about the desirability of joining their companies, but Daan’s aversion of Dolf van den Brink was a real stumbling block. I clearly remember how happy Piet was when he heard of Dolf’s plan to meet Daan and Bart at the Frankfurt book fair with the purpose to informally, but finally talk about a possible merger. He had begged him to be lenient. But apparently he hadn’t been. Bart hadn’t shown

of money, was certainly a good candidate for such a takeover, but after this speech Daan exclaimed: 'Never under Dolf!', and in September he wrote to Van den Brink:

Our ideas about the development of the concern [that will arise from the sale] differ to such an extent that, under the present circumstances, a construction that would satisfy both parties is inconceivable. At present it is not expedient to continue our discussions.⁶

A year passed in which Daan and Bart looked for alternatives. In Europe interest could only be expected from Pergamon Press in Oxford, and Springer-Verlag in Heidelberg. But whoever wanted to do business with Robert Maxwell? Daan referred to him, following the example of Per Saugman, as the 'bouncing cheque'—an allusion to his Czech (in fact, Ruthenian) origins and to his cheating practices in the 1950s, when they set up nuclear journals. There was no question, therefore, of Daan wishing to sell his pride and joy to him. Nor did he wish to approach Heinz Götze, the director of the thoroughly respectable Springer-Verlag, who had only very recently taken the step to publish in English. Strange as it may seem, for him Germany was one step too far; they would have to look around in America.

Did John Wiley & Sons show interest? Bart could easily have tested the waters with them, as he was a regular visitor of Brad Wiley in New York.⁷ Brad, John Wiley's descendant and director of the family business had taken over the distribution of North-Holland books in the United States and Canada from Interscience in 1963, when Maurits Dekker and Eric Proskauer sold their firm to him. And now, six years later, in the beginning of 1969, Bart spoke to Brad about the takeover of North-Holland and offered himself as ideal partner...but alas, no letters exist. Oh, how often do we lack documentation about what is most important! Brad had amicably told him that he was not keen on the idea, the argument being that it would stand in the way of his European branch in England if he took over North-Holland. Andrew Neilly, Brad's successor as president of John Wiley, would later say that

leniency either, according to Piet, because he saw little merit in Dolf's ideas and also because he was not yet convinced that North-Holland had to be sold.'

⁶ Letter from Daan Frank to Dolf van den Brink of 23 September 1968; this letter starts with a reference to their discussion at the Frankfurt book fair and ends with: 'We [Bart and I] hope that this decision will not spoil the good relations between us personally and our companies.'

⁷ Interview of Bart van Tongeren on 12 February 2002.

his refusal was the mistake of the century and that, in fact, Brad had another reason. He was reluctant to throw in his lot again with the Dutch. The Dutch—that could only mean father and son Dekker.

It was apparently his unfortunate experiences with these two that prevented John Wiley from buying North-Holland. We therefore decided to look up Marcel Dekker, the son, when we were in New York to visit someone else (Gerry Brown).⁸ Marcel recalled how he had set up his own publishing firm, Marcel Dekker Inc., in New York in 1963, after his father had sold his share in Interscience to Wiley and gone to work for Brad Wiley. He, Marcel, had not been involved with any of this and, thanks to a lucky speculation, had enough capital for a publishing firm of his own. At the time his father, a still very vital 66 year old, had left Wiley and become chairman of his board of directors ('Chairman Mao'). At meetings of the American Chemical Society he had then spoken to authors with whom he had previous dealings and suggested they could now also have their work published by Marcel Dekker—himself, in fact—and that they no longer needed Interscience, now in new hands. Marcel had even gone with his father to a lawyer to make sure that the contract, whereby Interscience was sold to John Wiley, did not exclude visits to the American Chemical Society.⁹

This is what Marcel told us in a frank and friendly discussion, but he rewrote the account of this discussion (sent to him for correction) because of its supposedly poor English, and in his much shortened version the last sentences were omitted.¹⁰ When we showed what had been scrapped to Bart van Tongeren, he held nothing back.¹¹ On 22 November 1963 he had an appointment with Brad Wiley. It was the day that President Kennedy was assassinated, so he remembered it exactly. Everyone was in a state of confusion, and Brad Wiley had asked Maurits Dekker, the father, to stand in for him. It was the first time that they met and, because there was little chance of their doing any business

⁸ Interview of Marcel Dekker on 18 March 2003.

⁹ Biography of Maurits Dekker by Marcel Dekker in *Journal of Macromolecular Science* **A26** (1989) nr. 8 pp. 3–6.

¹⁰ Letter from Marcel Dekker to the author of 20 May 2003; the shortened version of the text of our interview ends as follows: 'At the end of 1964 (*sic*) Maurits's son left the merged company (e.g. Wiley) to establish his own publishing house, Marcel Dekker, Inc. In 1966, Maurits was invited by his son to join his company as chairman of the board. He served in this capacity and specialised in editorial acquisitions until 1990, a career of sixty-seven years.'

¹¹ Letter from Bart van Tongeren to the author of 24 July 2004, p. 3; in a letter of 1 February 2006 Bart tells the story again in precisely the same words.

together, they talked instead about the plans of Marcel, the son, who had joined the staff of Wiley after their takeover of Interscience, but had suddenly resigned. 'Will he perhaps be setting up his own publishing firm?' Bart had asked. 'Yes', the proud father had let slip, in Dutch: '*maar ik rij niet mee op die wagen. Kom nou, ik zit mee op de bok!*' ('but I'm not climbing onto that wagon. Come on, I'm also going to sit on the driver's bench!'). This can't have been made up. So it looks as though Maurits, in setting up Marcel Dekker Inc., violated the rule that specific knowledge of a firm that is sold is not to be used elsewhere. Whoever breaks this rule loses his good name. So Maurits would not have 'just left' Wiley: Brad would have thrown him out.

Daan, too, must have looked into the possibility of selling North-Holland to Wiley. In May 1969 he was in Florence, where regulations regarding the International Group of Scientific, Technical and Medical Publishers (*STM*) were to be established.¹² He was treasurer of this interest group, which will be discussed in the last chapter of this book. Eric Proskauer, who had done the right thing and stayed with Wiley, was also in Florence representing their interests, and it is unthinkable that Daan would not have discussed with his old friend from Leipzig the obstacles that might prevent a takeover by Wiley. Eric would have known all about it. However, it is characteristic of Daan that we find no mention of the unsavoury 'Marcel Dekker Inc.' affair in his papers.

In June 1969, we see Piet Bergmans, who had been genuinely shocked the year before by Dolf van den Brink's clumsy advances, again approaching Daan Frank, very informally, in the so-called 'park bench consultation'. We were informed about this by Otto ter Haar, Bergman's close assistant and eventual successor, and about whom we shall say more at the end of this chapter. He writes:

Daan and Piet met up on their walks in Groenendaal, a large park close to Piet's home in Heemstede (a suburb of Haarlem). During these walks, spent largely seated on the benches rather than walking through the park, they thought out ways of eliminating the influence of Van den Brink, the cardinal problem for Daan. Piet knew that his own interests would be served as well; also he knew nothing of the discussions with Wiley. We at Elsevier can only be grateful that these discussions came to nothing, for

¹² Minutes of the Florence-meeting, to be found in box 1 of the *STM* archive. [In 2004 this archive was kept in a rented room of the Royal Library, The Hague; the complete archive then consisted of 28 numbered boxes and about the same volume of nondescript material.]

a combination of John Wiley and North-Holland under Brad and Bart would have made matters very difficult for us. We couldn't understand the fear of Dolf, however. We had lived for years in close proximity with him and he never really bothered us. He dealt mainly with other matters in the holding and understood little of our management pursuits. Those who had known him longer knew that he could easily be kept in check. But Daan didn't want to take any risks; he already had experience of Dolf wishing to isolate him and suggesting privately that they together, i.e. he and Dolf, carry on for another two years as general managers. But that would certainly have led to his being pushed aside. So he and Piet thought up a construction whereby Dolf's power could be kept in check. With the sale to Elsevier, which was to be presented as an amalgamation, the two science publishers would both become parts of a separate public limited company with its own name and a separate board of directors. Dolf van den Brink could be a member, but an outsider should wield the chairman's hammer. Bart van Tongeren would become director when Daan stood down, and then he would be admitted to the executive board of the holding company. I [Otto ter Haar] would become co-director when Piet stood down; they'd thought that out as well. Daan would have to sell the whole idea to Bart, and it was up to Piet to convince Dolf. It couldn't have been too difficult, even though Bart must have had mixed feelings at the prospect of grappling with the bear. Dolf, who was not entirely unrealistic, understood why he was being held at a distance. As president-director he would anyway have the last word. Nor did he make a fuss about the purchasing price.¹³

The problem 'Never under Dolf!' could, therefore, be solved. Logically enough—it wasn't a matter of squaring the circle, but at most of non-linear programming, something which Guus Zoutendijk, a Leiden professor of mathematics, excelled at; he was prepared to chair the executive board. Since the publication of his *Methods of Feasible Directions*, Piet valued him highly and the others were familiar with his social and political interests. The communiqué of September 1969 that we mentioned at the beginning of this chapter stated that branches of Elsevier abroad (in Lausanne, London, Mexico, New York and—later—Tokyo) would also be included in the new plc. No mention was made that Reidel in Dordrecht, which was owned effectively by North-Holland, would remain outside the agreement as Anton Reidel had stipulated that he

¹³ Letter from Otto ter Haar to the author of 15 November 2005; Otto's story goes back to stories told to him by Piet Bergmans, whereas the detail on the deal proposed by Dolf van den Brink to Daan Frank goes back to Anneke Frank-van Westrienen. Incidentally, Dolf had also offered the co-directorship of the joint company to Bart van Tongeren if the latter agreed with a take-over by Elsevier (interview of Bart van Tongeren on 12 February 2002).

be allowed to extricate himself should Daan Frank wish to sell his firm. The merging together and separation of the various companies took an unexpectedly long time, due to misplaced attempts to economise and a failed attempt at automation that had reduced the central administration of the Elsevier holding to chaos, and the man who was to restore it all to order had not yet been appointed.¹⁴ Jan Verleur—later taken onto the management board—worked so hard at it all that he, often literally, slept in the office. And so it came about that the plc could only be set up on 12 March 1970. And its name: Associated Scientific Publishers. Director: Engelbart van Tongeren.¹⁵

The association that was to lead to integration threatened to become burdened with *Excerpta Medica*, the international documentation service that we discussed in the previous chapter. In 1971 Dolf van den Brink had also decided to buy this Foundation—converted again into a plc to this end—impressed as he was by the successful application of the computer they used there to document scientific articles.¹⁶ He even

¹⁴ Letter from Bart van Tongeren to the author of 24 July 2004, p. 16: 'North-Holland had a simple manual reporting system, where one could see at the first glance for which number of sold books the break-even point was reached—to give just this example. Elsevier's reporting system was less conveniently arranged and partly computerised under the appropriate name 'Oracle'. When in the fall of 1970 these systems were united, 'Oracle' turned out to be unable to produce North-Holland's bills for the renewal of journal subscriptions. We had to make them by hand, as before. But not only the program, also Elsevier's computer that had cost 4 million guilders turned out to be unfit for the reporting and needed to be replaced. For this replacement and the further computerisation of the reporting a specialist had to be attracted.' This was Jan Verleur, about whom Otto ter Haar said the following in the interview of 11 April 2003: 'For the thrifty Dolf van den Brink everything had to be on the cheap, and as a result everything in Elsevier was shaky, not only the chairs and desks and cupboards, but also the reporting system. When Dolf saw that everyone was buying computers for the purpose he finally bought a cheap one—the wrong one. Then his reporting became a drama. To save Elsevier from chaos he needed the experience and energy of an outsider. So he bought Jan Verleur from an industrial firm in the East, a young man who had worked for IBM. Verleur's tedious drudgery to bring order in the central administration was rewarded, and when he later also had given proofs of his insight in fiscal tricks he got a position on the board of directors.' On 24 November 2005, Otto ter Haar yet wrote the following about him: 'Verleur really knew how to organise and he really was inexhaustible, but sometimes he needed a little sleep and then he slept in the office.' See also Vermeulen & Wit p. 36.

¹⁵ We don't know the precise date of the memorandum of association; 12 March 1970 is the date on the balance sheet, signed by J.H. Verleur (see previous note), where shortly (?) after the juridical association the shares of North-Holland and of the dependent companies outside the Netherlands were booked (the start capital was 21,356,203 guilders).

¹⁶ The take-over is announced in a *Communiqué* of 11 February 1971, in which it is stressed that the experience of *Excerpta Medica* with advanced methods of data

wanted Pierre Vinken, director of *Excerpta Medica*, on the board to show them at Elsevier how to implement such applications. But for a number of reasons, the most important being that this service published only secondary literature, it was excluded from the Associated Scientific Publishers with their primary literature. A board member of *Excerpta Medica* described Vinken as follows:

Small, slim, in fact thin as a rake; he was quick in his movements and even quicker of mind. I have never met anyone who could turn his thoughts into action with such ease. A mind that thought in straight lines, belonging to a man who [...] from the “ballast” with which he was crammed at the universities, was able infallibly to pick out what he needed.¹⁷

This man had such powers of persuasion that Van den Brink was prepared to pay almost as much for ‘his’ medical documentation service as for North-Holland, although the turnover and especially the profit margin were clearly smaller. Dolf’s accountants had their doubts about the transaction. But the use of a computer for files and records or for documentation was still quite new—the first, very bulky mainframes of electronic office machines date from the second half of the 1950s—so for Dolf, experience with such a machine seemed extremely valuable. Let us not forget that in the 1960s the training of engineers in numerical analysis and programming computers was still in its very early stages!¹⁸

processing will be of great importance for the future of the Associated Scientific Publishers; the three companies together (Elsevier, *Excerpta Medica* and North-Holland) employ 500 personnel. See further Vermeulen & Wit p. 35; Amerongen p. 37; note 29 of Chapter 6. Otto ter Haar writes in his letter of 8 September 2004 to the author: ‘Insiders were astonished by the back-transformation of *Excerpta Medica* in a limited liability company, of which the directors were considered to be the only owners, but Pierre Vinken got away with the trick and cashed ‘his’ millions. One cannot say that he cheated Dolf van den Brink in his vision that automation and computers were Elsevier’s future, but *Excerpta Medica* never proved to be the financial success he had prophesied. The profit margin of its journal was small, in contrast to that of ordinary science journals, since one had to pay for the making of excerpts and the editorial control, while the text for the ordinary science journals was free (and basically paid by universities and science institutes), and moreover, the competition of other abstract services turned out to be severe.’

¹⁷ Amerongen p. 35.

¹⁸ We recall the pioneering work of Egbert van Spiegel in Delft and Guus Zoutendijk in Leiden, who started their courses in numerical analysis and computer programming in 1960 and 1964, respectively; both became politically active, in the socialist party and the liberal party, respectively, and both became top-manager, of a new Ministry of Science Management and of Delta Lloyd (an all-round insurance company), respectively, while the latter has been mentioned already as chairman of the board of the Associated Scientific Publishers.

And indeed, although we should not refer to knowledge of data files and software as “ballast”, Vinken had managed to extract from it what he needed. While James Cauverien kept the excerpts factory going, Vinken looked for ways to automate it. Pioneering was unnecessary, it seemed. In 1965 the software engineer Frans van der Walle had explained how literature files could be opened by way of computers, and then Vinken did not hesitate to buy them (two NCR 315–501s with rod memory), and assign to Van der Walle the task of developing an automatic information system (Mark I) for the excerpts.¹⁹

When in 1962 Vinken succeeded Van Tongeren as director of Excerpta Medica, there already lay on his desk a ‘1000-journal programme’ to modernise the international documentation service with the help of computers. Obviously specialists from Europe and America had even got together in Amsterdam to discuss this future form of the service.²⁰ On this occasion the director of the American medical library in Bethesda, Frank Rogers, had pointed out that in America they already had an *Index Medicus*. Two years later, in early 1964, a Honeywell 800–200 computer was up and running in Bethesda for the documentation. The information system that Pierre Vinken had developed in 1965 and 1966 would not, therefore, have differed greatly from the *Index Medicus*. That of Excerpta Medica was based on terms that occurred in medical texts, with an added vocabulary control mechanism. This control was based on a thesaurus, a long list of terms with exact descriptions of their meanings, and their synonyms, which he had drawn up by his assistant, Robert Blanken. The system was ready in early 1968, which Pierre promptly announced in the *Nederlands Tijdschrift voor Geneeskunde*:

In recent years Excerpta Medica has completely automated its information system. Per year 150,000 biomedical publications, divided across 34 specialisms, classified according to a system of 3000 categories, are provided with keywords and then stored in the computer memory. The 5 to 10 index terms per article are checked by the computer, i.e. synonyms are automatically replaced with preferred terms by a translation program in the computer, so that the same concept is always indicated

¹⁹ Fredriksson pp. 166–167 (in the Chapter by Robert R. Blanken and Pierre J. Vinken).

²⁰ Fredriksson p. 168; see also E. van Tongeren, ‘Minutes of the International Meeting on the Modernisation of Medical Information, held in Amsterdam on 15–17 May 1962’, Excerpta Medica Foundation, Amsterdam-London-New York (June 1962).

by the same term. The translation program works with a thesaurus of around 150,000 terms.²¹

All this information, which also became available on tape, appeared in the Abstract Journals of which Excerpta Medica then published 34 each month. In the year that Vinken sold the enterprise to Van den Brink he was able to add 6 more, including *Environmental Health* and *Health Economics*, specialisms that were then in their infancy. When he was then given the task to work out a strategy for the modernisation of Elsevier as a publisher of scientific literature, he thought about problems of scientific communication in the general sense, and of the course taken so far by these Abstract Journals.²² (We remember how thick they were at the time, the ‘telephone books’ with abstracts published by the American Physical Society! In the period of the 1950s that we describe 50% of these and similar books were on physics, 25% mathematics, 15% biology and 10% on chemistry.)²³ According to Vinken the ‘ordinary’ journals had outstripped their purpose, because they had become too copious and specialist, and the future would lie in information gathering. All articles—not only medical—would have to be documented in computers.

But even if it were true that after the takeover of the two publishers Elsevier had sufficient editors and authors at its disposal to set up a database of global interest, it didn’t mean that the commercial interest therein would be enough to justify it. Certainly it would be to the advantage of researchers. Ever since the 1960s they had leafed through the unsightly American booklets known as *Current Contents*, to see if perhaps they had missed out on something. Unsightly or not—in fact outward appearances were unimportant, for the astute Eugene Garfield, their publisher, was fully aware that insecure brethren would pounce on them. But publishers? In practice the only concrete result that came of this vision on information gathering was a special professorship for Pierre Vinken at Leiden University. It was created at the instigation of the already mentioned Guus Zoutendijk who was director of the University’s computer centre and wanted to foster the use of computers

²¹ P.J. Vinken, ‘Mogelijkheden voor het ontwikkelen van een geautomatiseerd pathologisch-anatomisch diagnosesysteem’, *Nederlands Tijdschrift voor Geneeskunde* **112** (1968) 2168.

²² P.J. Vinken, *Long-term Development of Scientific Documentation* (Albertina Boot, Ed.), brochure Dutch Unesco Committee, The Hague (1972).

²³ Abel & Newlin p. 10.

in the medical faculty. Vinken accepted it in 1976 with an address on scientific information as a unique commodity, a commodity with the property that it seems to generate itself, independent of its content.²⁴ Derek de Solla Price had given examples in his book *Little Science, Big Science* of 1963, and the above mentioned Eugene Garfield had set up an Institute of Scientific Information in Philadelphia.²⁵ Trivial features in scientific publications seemed to suggest that a non-trivial science of science was possible. The number of publications of a researcher on a particular subject and the number of times that these publications were cited by others could be a measure of their significance. Was it necessary to distinguish any longer between quantity and quality, or between information and knowledge?²⁶ When we raised this key question of Vinken's professorship, it was impossible to refrain from saying that scientific knowledge can only be based on objective facts that can be verified by all, not on information in the form of texts and theories about these facts—which can, after all, be wrong.

'I don't agree', he said: 'That interpretation is too limited. But don't let's quibble about what is the meaning of science.'

'The science of science—that is a meta-science. How do you lecture in that?'

'I didn't give lectures.'

'How strange. Aren't lectures the only task required of a professor by law?'

'That's true, but I was supposed to be expanding Elsevier. When I was invited for that professorship, I said clearly that I wouldn't have time to give lectures. The gentlemen agreed.'

'Didn't it entail any obligations?'

'They included me in plans for the medical faculty and asked my advice in matters concerning management.'

²⁴ P.J. Vinken, *Informatie genereert informatie*, Excerpta Medica, Amsterdam-Princeton-Genève-Tokio-Melbourne (1976); at the end of this inaugural address on the properties of information the speaker admits not to know what information is precisely: 'Perhaps it isn't even possible to speak of an information science.'

²⁵ Price *passim*; for Garfield, see note 73. Sociological aspects of the recent growth of scientific knowledge have been discussed by Merton *passim*; Merton commented on the ambivalence of scientists in this growth process in: *Synthese Library* 99 (1976) 431–455, where he says a.o.: 'The scientist should make every effort to know the work of predecessors and contemporaries in his field, BUT too much reading and erudition will only stultify creative work'—and thus shades the importance of information; for Merton's impact on the understanding of real science, see Ziman pp. 31, 155.

²⁶ Interview of Pierre Vinken on 1 October 2004.

Which is to say that in science, too, things were no longer as they had been up until then. With the merging of the publishers the boundaries between disciplines within which they had specialised, chemistry and physics, and the biomedical science that relied upon them, came to lose their meaning. Even in their content these disciplines were in the process of coming together.

Dissolution of Boundaries

A few journals published by Elsevier in the 1960s serve to show how boundaries between disciplines became blurred. Apart from *Biochimica et Biophysica Acta*, we have not yet discussed the science journals of Elsevier during this period. It is time to give them our attention, if only to understand where Van den Brink found the money to pay Frank and Vinken and company for their publishing houses. Let us compare this money to the sludge that settled after the deluge, in which—amongst others—neurology, chemical analysis and knowledge of the earth's crust appear. For isn't it just such an image that thrusts itself upon us, in which all is turned upside down and mixed together? Such is multi-disciplinary science. In the appropriate order we shall discuss *Brain Research*, the *Journal of Chromatography*, and *Tectonophysics*—three of the 54 journals that Elsevier published at the time of the amalgamation.

The idea for a journal on brain research came from Johannes ('Jos') Schadé, a neurologist from the Brain Institute in Amsterdam. In 1965 he managed to convince Jacques Remarque that researchers had been waiting for just such a journal.²⁷ If Elsevier didn't publish it, then another publisher would do so. Jos felt that he was still too young for the task, and too unknown, but he knew plenty of people and was sure that Konrad Akert in Zurich would probably like to edit it...At

²⁷ Interview of Jacques Remarque on 28 May 2003. Already prolific as an editor, Schadé had then just edited, together with John Eccles, 'Organization of the Spinal Cord' in: *Progress in Brain Research* **11** (1964) 285 pp., a series published by Elsevier; he was about to publish in *Brain* (an old-fashioned journal of MacMillan), together with Konrad Akert and Claus Bally on problems of dynamic neurology, as well as, together with Donald Ford, an entire book: *Basic Neurology—An Introduction to the Structure and Function of the Nervous System* (Elsevier, 1965): So Schadé knew the great neurologists and his fertile brain was full of ideas; in 1969 he would give a brilliant proof of the existence of virtual perception by his so-called radio-report of the Moon landing from NASA headquarters in Houston, while he spoke from a call-box in Amsterdam.

Elsevier, however, there was serious doubt about the viability of a journal that focussed on an object—an organ—and not on a subject—a discipline such as, for example, neurophysiology. Nor did people have any idea of the great boom in brain research worldwide. They only saw the modest institute of Bernard Brouwer, set up in Amsterdam in days gone by, at the beginning of the century, when the Spaniard Santiago Ramón y Cajal discovered that the nervous system consists of independent cells, and the Englishman Charles Sherrington had seen that these cells (neurons) were attached to one another by thread-like extensions (synapses).²⁸ But only now, fifty years later, could brain research really start to flourish: according to the enthusiastic Schädé, who worked on a sceptical Remarque. And this came about thanks to the drastic improvement in diagnostic techniques, which in turn came from the tremendous development of semiconductor electronics in the 1950s.²⁹ These techniques, which included electron microscopy, made the research multi-disciplinary. For the first time encephalography with electrodes and X-rays could provide real insight into brain function and into the highly complex structures of the central nervous system.³⁰ Then there were also electromyography and arteriography, which showed how muscles were put into motion, and how the heart and circulation system worked, how the nervous system was provided with blood, etc. Remarque had also seen from ‘his’ *Biochimica et Biophysica Acta* that effective pharmaceuticals were available for migraine and epileptic attacks, just to mention these two particular diseases of the brain.

So Remarque went to Akert, director of the *Institut für Hirnforschung* at the University of Zurich, to discuss the publication of a journal *Brain Research*. He was almost too late.³¹ Akert had only just agreed to set up the journal for Elsevier, when John Eccles, Nobel Prize winner for physiology in 1963, invited him to edit the journal *Experimental Brain Research* that Springer wished to publish. ‘But I stayed with the Dutch,’ the Swiss told us when we visited him many years later in Zurich to talk about the creation of *Brain Research*:

²⁸ Oeser pp. 214–217; Popper & Eccles pp. 229–235.

²⁹ Seitz & Einspruch pp. 164–211; see also the Editorial Note with which *Brain Research* was launched in 1966: ‘The trend toward multidisciplinary investigations of brain functions has led to the introduction of a number of *new and powerful research tools* and the involvement of an ever growing number of workers.’

³⁰ Oeser pp. 244–250.

³¹ Interview of Jacques Remarque on 28 May 2003.

Yes, Remarque asked me to set up the journal. I found it a good idea, because we really needed something like that. The East German *Zeitschrift für Hirnforschung* was without any significance. We read *The Journal of Physiology* by Sherrington, the man who discovered the synapse, but that journal was too limited. I don't know why Remarque asked me to do it. Not because of my book on the comparative anatomy of the frontal cortex. I think that I was fairly well-known by then. I was struck by the fact that the invitation came from Holland. My father was born in the Dutch East Indies, on Sumatra. Did he know that? I knew Bernard Brouwer from the Brain Institute in Amsterdam, also Jos Schadé, the man whose life took such a strange turn. When Eccles approached me, which I certainly regarded as an honour, I felt a certain loyalty towards Holland. My honeymoon, a year before I took my doctor's degree, took me to Amsterdam. While my wife ambled round the Rijksmuseum looking at their collection of seventeenth century paintings, I visited Vrolik's anatomical cabinet to look at their collection of fish brains!³²

Then he told us about the effect which this collection of fish brains had on his subsequent career. In 1947, at 28 years old, he did his PhD under Walter Hess on the visuomotoric system that is to be found in the centre of the brain of a trout, carrying out an electro-physiological and histological study of this centre. His tutor was a generalist, but well-known for his study of the nervous system, especially of the brain stem, for which he would be awarded a Nobel Prize in that same year, 1947. Naturally Akert was familiar with his method of stereotactical stimulation. The Nobel Prize winner, Ragnar Granit, had also used this method to study visuomotoric systems and higher brain functions. But micro-electrodes could also be applied, as Eccles did when he proved the electrolytic character of the synapse, or electro-encephalograms recorded, as Edgar Adrian did when he discovered brain waves. Long before he went to university Akert knew what kind of research he was born to. His boyhood adventure stories were the writings of the Russian Constantin von Monakov who, half a century earlier, had set up an institute for brain research in Zurich, his birthplace, and had discovered the nerve cords and structures that are now named after him.³³ Also he had been considerably inspired by the books of Cécile and Oskar Vogt, Germans who were well known for their research on higher brain functions and for their open criticism of the Nazis.

³² Interview of Konrad Akert on 9 December 2003.

³³ Oeser pp. 219, 226.

After obtaining his PhD, Akert went with his family to America. First he was research assistant in physiology at the Johns Hopkins University in Baltimore, where he worked with Vernon Mountcastle, later to become internationally famous.³⁴ After two years he obtained a place at the University of Wisconsin in Madison, where he found in Clinton Woolsey an inspiring tutor and pillar of support for the rest of his career. He also learnt a great deal from Jerszy Rose, originally a Pole, and from Walle Nauta, originally a Dutchman.³⁵ In 1961 he was asked to return to Zurich so that he could upgrade brain research there, and set up an institute for that purpose. At the time ‘everyone’ was trying to discover the relationship between anatomical and physiological processes. He employed young people who had not only been trained in anatomy and physiology, but also in chemistry and psychology. Naturally he needed to procure modern apparatus, such as an electron microscope and an electro-encephalograph, and he continued to develop the research methods that he had learnt in America. Working together with Woolsey meant an exchange of post-doctoral students, and money became available from the National Institutes of Health in Bethesda. He also got money from the pharmaceutical industry in Basel; and in exchange an extraordinarily interested man, Alfred Pletscher, leader of pharmacological research at Hoffmann-La Roche, was allowed to observe his studies of the connections between the brain stem and limbic cortex—observe, not direct. Akert saw the social dimensions of his work, and in 1962 let himself be elected to the Zurich canton council. After one term he resigned, because parliamentary work took up more time than he had expected. Which brings us once more to the history of the journal:

I had negotiated that the editorial secretariat of *Brain Research* would be with me at the *Institut für Hirnforschung*, and that it would be paid for by Elsevier—Barbara Vannotti dealt with this for years. Zurich was a good place for it. Switzerland is a neat little country, is it not? The trains run punctually, the post is delivered on time. Elsevier paid me as well, but I distributed the money to the staff that helped me at the institute. Their most important task was to build up and keep up to date lists of experts in the disciplines that covered brain research—referees who we could

³⁴ The great contributions by Vernon Mountcastle and Walle Nauta to the knowledge of the cerebral cortex and conscious perception are mentioned in Popper & Eccles pp. 225–274.

³⁵ Popper & Eccles pp. 225–274.

consult. My most important task was in the beginning. The journal had to be well anchored within international research, which meant making countless telephone calls and writing long letters. Pierre Buser in Paris did his utmost to persuade his French colleagues to send their articles to *Brain Research*. There were 35 members in the editorial team that I put together, and 20 of them were from outside Europe. [In 1980 there would be 71, and in 2000 as many as 81.] I had managed to get hold of Eccles from Canberra, also Nauta from Cambridge (Massachusetts), as well as a great organiser, Dominick Purpura from New York, who took over from me in 1976 as main editor when, after ten years, I'd had enough. Jos Schadé was co-editor for the first couple of years, but already then I usually dealt with most of the articles myself. I determined to always stay honest and friendly, never become personal and never get into rows with the cliques. *Brain Research* ended up serving some 25,000 researchers, and three organisations: the IBRO (International Brain Research Organisation) in America, the EBBS (European Brain & Behaviour Society) and the ANA (American Neuroscience Association). In the 1980s the latter brought about an explosive growth of the journal, and then came a burst of related journals. And as far as *Brain Research* is concerned, it has now reached its thousandth volume. It could even be the largest journal in the world. But the *Journal of Neuroscience* is better.³⁶

This is how this friendly fellow looked back on his brainchild. We were sitting in a busy room of the Zürcher *Hauptbahnhof* and wine had been poured. 'Take a piece of bread first,' he said, 'then it tastes better.'

Now we come to the history of the second journal, the *Journal of Chromatography*. Outsiders will understand that chromatography has to do with colour (the Greek *χρῶμα* means colour), but as a rule it is only chemists who know that 'colour writing' refers to a specific method to determine the chemical composition of a substance. This method dates from the late nineteenth century—use of Greek terms already betrays its great age—but it is still important, and works so well that the end of its development is not yet in sight.³⁷ It is based on the differences in the strength with which molecules attach themselves to an inert surface. When a mixture of unknown substances, each with its own specific molecules, is converted into fluid (or gaseous) form, and is allowed to flow from a fixed point over an inert surface, we can expect the molecules with the weakest potential for attachment to flow the furthest. After a while the different molecules, belonging to the components of

³⁶ Interview of Konrad Akert on 9 December 2003.

³⁷ Ettre & Zlatkis pp. vii–ix; this book contains Lederer's paper to which the next four notes refer.

the mixture, will end up at various positions along the path over the surface. When they are deposited, they form stripes perpendicular to the flow direction, and since these stripes sometimes have a characteristic colour the component in question can be directly recognised—that is to say: the colour writes the name. The first person to try out something like this was the Russian botanist Mikhail Tswett. And because he understood how important it was that the surface was indeed inert, he succeeded in isolating chlorophyll from a plant extract. His book in which the method is described dates from 1910, but is unfortunately in Russian and remained for a long time in obscurity, also because it seemed to be of interest only to botanists. The method was rediscovered in 1931 by Edgar Lederer, an Austrian chemist.³⁸ After fundamental research into the possibility of separating not only fluid mixtures, but also gaseous mixtures conducted over an inert surface, an article in 1952 by Anthony James and Archer Martin served as the impetus towards a fast industrial development of this ‘colour writing’. That same year Martin was awarded a Nobel Prize for chemistry, together with Richard Synge. These last three were all British. The nephew of Edgar Lederer was also involved in this development. In 1956 Michael Lederer, 16 years younger than Edgar, succeeded in separating chromatographically a colloidal solution of resins, while at the same time separating the radioactive lanthanides that were adsorbed into the resins with the help of ion exchange.³⁹ This proved that the method could be applied not only in biochemistry and in organic chemistry, but also in nuclear chemistry. Surely such a universal method that evidently transcended the boundaries of disciplines required its own journal?

Michael Lederer, who set up the *Journal of Chromatography* in 1958, was born in Vienna in 1924. Because the Lederers were Jewish, after the annexation of Austria by Germany he and his parents went to Australia. His uncle Edgar had already found work in France, became naturalised, and managed to escape the round-ups under the German occupation. In Sydney, at a safe distance from these cruel practices, Michael studied chemistry. Here he learned how to isolate metals and in 1948 published an article on this subject in *Analytica Chimica Acta*. When, a short time later, he published an outline on chromatography,

³⁸ Idem pp. 237–238.

³⁹ Idem pp. 247–253, in particular p. 250: ‘We had thus described the first ion-exchange resin paper that was useful for chromatographic work in 1956.’

Edgar invited him to come to France so that they could work together from then on. Michael did not have to be asked twice, and in 1951 he arrived in Paris, where Irène Joliot-Curie took him on as an assistant at the *Institut du Radium*, to study the separation of radioactive elements. For this he was awarded a doctorate three years later at the Sorbonne; in the meantime he also helped Edgar with an English republication of his book on chromatography. He even had time to write his own book, *Introduction to Paper Electrophoresis and Related Methods* that was published by Elsevier in 1955, as was their joint *Chromatography—A Review of Principles and Applications*. Uncle and nephew had contacted Elsevier when they were wondering who could publish their books in English and, on going through the bookcase came across *Organic Chemistry*, Paul Karrer's textbook that Elsevier had once published so beautifully.⁴⁰ It was on the occasion of the publication of their books that Michael suggested to the director, Piet Bergmans, that they start up a journal on chromatography:

When I came up with this idea, Mr Bergmans wasn't at all enthusiastic, and just kept asking questions. Was chromatography here to stay? And wasn't there already a journal for analytical chemistry? [As we know, Bergmans had already tried to sell *Analytica Chimica Acta*.] But he only told me the real reason at a dinner given on the occasion of the hundredth volume of the *Journal of Chromatography*. At the time science publishing stood deep in the red with the Elsevier holding, and the president [Van den Brink] would certainly not have agreed to a new journal. Naturally he couldn't tell me this. But in 1957 there was a breakthrough. Mr Bergmans had been to the International Council of Scientific Unions in Paris and been asked to publish their Review. This success prompted him to telephone me. We met up at the Café Weber at the Madeleine. He was in such a good mood that I brought up the subject of the journal again. Now I can afford to take a risk, he must have thought, because he agreed to it straightaway. I should discuss the editorial details with Dr Gaade.⁴¹

Michael must have felt that he had *carte blanche*, and that he had no need of the experienced Willem Gaade to make a good job of editing the journal in the proper style. In 1960, after he got 'his own' laboratory, the *Laboratorio di Cromatografia* in Rome, he let there be no doubt as to who was the boss. The correspondence that arose between them both was full of suspicion, and only became less frosty when 'Maru' and

⁴⁰ Idem p. 249.

⁴¹ Idem pp. 251–252.

‘Puntje’ entered into it, their two cats that must both have been possessed of great character.⁴² Willem, who went red-faced at the slightest mistake he discovered in an Elsevier publication, must have died of shame when he was obliged to approve Lederer’s Editorial in the first issue of the *Journal of Chromatography*. Anyone who opens a new journal and states its aims, chooses his words with care, has his text checked and puts his name under it. But this anonymous mishmash... Not a word about the importance of the method. Referring to journals, just like that, in the field of electrochemistry, microchemistry, spectrochemistry and nuclear chemistry, in which up until then articles had appeared on chromatography and could from now on be disregarded... And then to show, in an absurd sequence, how the new journal was open to theoretical questions of structure etc., for practical questions such as the most recent physicochemical constants, and that articles on electrophoresis were especially welcome, as well as reviews etc.⁴³

However, when we look back on the 26 years during which he led the journal, we see that Lederer did achieve his goal. His *Journal of Chromatography* would become one of Elsevier’s greatest journals, and on his departure was publishing 25 volumes per year. Of the 250,000 articles that were published worldwide on chromatography between 1940 and 1980, from 1960 onwards he published 20,000, and around 1980 served a quarter of the chromatography market.⁴⁴ Is it so strange that he came to consider himself as owner of the journal? At any rate it could be called the intellectual property of an ever increasing number of researchers—of which he was not the least—who explained in its pages what could be separated by the differences in attachment potential or electrical charge, after ion exchange or complex formation, or whatever preliminary treatment was needed for the material being investigated. Added to this is the fact that he edited it on his own for the first ten years. Only in 1969, when it took 7 volumes to publish all the material and the work burden became so great, did he request assistance. Karel Macek, a Czech who came to work for him for a year at the *Laboratorio di Cromatografia*, took over the editing of the section for pharmaceutical and medical applications, and Willem Gaade had

⁴² Interview of Marc Atkins on 16 August 2005.

⁴³ *Journal of Chromatography* **1** (1958) i; not Willem Gaade, but the author is responsible for this judgement.

⁴⁴ Data from Karel Macek’s paper on the essential role of Michael Lederer in the founding and initial editing of the journal: *Journal of Chromatography* **313** (1984) 1–2.

already long handed over production of the journal to Marc Atkins, together with its laborious correspondence.

Marc Atkins came to work at Elsevier in 1960.⁴⁵ He was born in Coventry in 1933, studied physical chemistry at Leeds, and completed his studies in 1958 at Aberystwyth (Wales) with a thesis on *The Mechanism of Hydrolysis of some Aryl Chlorides*. For a while it looked as though his future would lie with Imperial Chemical Industries, but the work there didn't suit him. He had greatly enjoyed a visit to Amsterdam, so replied to an advertisement in *Nature* in which Elsevier asked for an editor in chemistry. He was received at the office in the Spuistraat amidst great expectations, also because he was only the second person there to speak and write perfect English.⁴⁶ The editing team, which up until then had consisted of only three subject specialists (Gaade, Meijer and Remarque) was enlarged to five, not only with Marc Atkins but also with Arie Manten, who will be discussed later. In 1964 Gaade found that Atkins, who worked for him, was sufficiently well versed to be able to publish the analytical- and physical-chemistry journals on his own (the organic- and inorganic-chemistry publications remained with Gaade). So in 1964 Marc Atkins had to get along with Lederer. We asked Atkins to tell us about this somewhat unusual relationship between scientist and publisher. He arrived on the stroke of the clock for our rendezvous in the first class restaurant of the Central Station in Amsterdam, dressed in a two-piece light grey suit.

I've lived for so long in this city, but I've never been here, he began, 'What a beautiful room! If I took guests out to lunch, I used to drive out to *De Bokkedoorns* in the dunes near Haarlem, or to *De Hoefslag* in the woods near Utrecht, both well-known places to eat. I must have taken Lederer to lunch there; his journal was important enough. I don't remember much about it any more, but it certainly wasn't particularly

⁴⁵ Interview of Marc Atkins on 16 August 2005.

⁴⁶ Letter from Otto ter Haar to the author of 4 August 2005: 'In contrast to Willem Gaade, Marc Atkins was discreet and business-like, fast and exceptionally intelligent. He stayed somewhat aloof, being an observer rather than a participant, with a clear aversion of the Dutch self-sufficiency and of the two 'buffoons' at the top. After the untimely death of Gaade he started the exploitation of Gaade's journals in a rigid, almost shameless fashion, and he also purged Gaade's book series by stopping some—for instance the beautifully edited Nobel Lectures, which were a financial disaster—and accelerating others, which mainly meant the chasing of authors who were late. In Atkins we missed a bit Gaade's visionary initiatives, but he was able to attract some good journals—for instance *Carbohydrate Research*, with editors in England and America, for which he had to act more quickly than Academic Press.'

pleasant. The man was unbearable. After the sudden death of Gaade—in 1970, three years before he was due to retire, due to a complication in an abdominal operation—I had to brave him on my own. With the growth of the journal in the 70s and 80s Lederer's fanaticism to keep everything under his own control reached ever greater heights. He wanted to do far too much himself. After the umpteenth break-in at his villa in Rome, he went to live in Sils Maria near St. Moritz in Switzerland, which meant that the secretariat in Rome couldn't step in any longer and I was faced with excessive demands for the editorial support. It was, anyway, ridiculous that a large journal should be edited from a small village. For years more than half the post that came to Sils Maria was for the *Journal of Chromatography*. Lederer's arrogance inevitably led to conflicts, and not only with us in Amsterdam. He didn't hesitate to offend authors, even to insult them. He took exception especially to Germans. I wanted to have German chromatographers in the editing team, but he was quite simply against it. Whether that had anything to do with his childhood as a Jew in Vienna, I don't know. For me only scientifically based arguments counted. In the end I... Well, how shall I put it? In the end I had come to an arrangement to have him replaced. It was not to be avoided. Karel Macek from Prague, the editor of the section on Biomedical Applications, other section editors and the regional editors cleared the path for a smooth changeover to a new editorial structure.⁴⁷

There is nothing particularly unusual about the manner in which the great interdisciplinary journals on a *method* and an *organ*—the brain—came into being. However, the way in which the interdisciplinary journal on a *discovery* came about is definitely unusual. For this discovery we return to 1960, when a geologist from Princeton was able to explain why the bed of the Atlantic Ocean half way between the continents is covered with only a thin layer of sediment, namely at the point where it rises up in a ridge.⁴⁸ The fact that this layer is thin, and therefore young, was discovered in the 1950s, when the deep ocean was studied in detail for the first time for the purpose of nuclear submarine operations. According to the geologist Harry Hess, new earth crust was formed at this point, due to the solidification of slowly flowing rock from the mantle immediately beneath it. The constantly widening ridge in the middle of the ocean must push the ocean bed on the left and the right of it sideways, thus moving apart the continents. Although

⁴⁷ Interview of Marc Atkins on 16 August 2005.

⁴⁸ Krige & Pestre pp. 391–416 (Chapter by Ronald E. Doel), where the hypothesis of Harry Hammond Hess is described on p. 394; see also Nye pp. 538–557 (Chapter by Naomi Oreskes and Ronald E. Doel) and Pannekoek pp. 111–126 (Chapter by Antonie J. Pannekoek).

it still remained unclear what caused the slow-moving convective flow in the upper mantle, Hess' explanation lent support to a hypothesis of Alfred Wegener from the beginning of the century. Wegener had attributed significance to the remarkable fit of the east coast of South America into the west coast of Africa, as if earlier these continents had lain up against one another, thereby solving the puzzle of how, in the distant past, continents could have lain up against one another and have moved away from one another. His hypothesis of shifting continents was disputed, however. Hess' explanation, moreover, showed why the seabed sometimes seemed to plunge downwards beneath the edge of continents, as Felix Vening Meinesz found out in the 1930s while studying troughs in the Indonesian archipelago, and as Hess found out himself in the Caribbean.

The correctness of Wegener's hypothesis could no longer be doubted when strong proof was provided in 1962 by Keith Runcorn from Newcastle-upon-Tyne, in collaboration with researchers in North America.⁴⁹ For what did they find? The weak magnetisation found in ancient rock, which could only be caused by the magnetic field of the earth, pointed to a different magnetic pole in England than in North America: For England the track which this pole travelled in the course of time seemed similar to that for North America, but both tracks diverged! And because the earth's magnetic field coheres uniquely with the earth's rotation, the divergence must be a reflection of the way in which the Atlantic Ocean has opened up since the Cambrian (500 million years ago). Of all that was added in the 1960s to this proof of the shifting plates of the earth's crust (continents), we mention only the identification of crust fragments, which often cause quakes because they move comparatively quickly along the edges of the plates—Tuzo Wilson had predicted their existence in 1963.⁵⁰ Even so, this new vision on the forces behind the shifting, folding, sinking and rising of parts of the earth's crust was not immediately shared by all. Vladimir Belousov from Moscow, for example, the patriarch of Russian geologists, would come up with counter arguments for years to come. Clearly geologists

⁴⁹ Pannekoek p. 115 (where a map is shown).

⁵⁰ Krige & Pestre p. 395: What transformed Hess' hypothesis of sea floor spreading into a comprehensive theory of crust movement 'was the confirmed existence of a new class of geological faults, called transform faults, which the Canadian geophysicist J. Tuzo Wilson had predicted in 1963.'

in the 1960s had much to discuss and had questions especially about the convection in the upper mantle.

These fascinating discoveries emerged bit by bit, in a hotchpotch of journals from scientific societies in diverse countries. There could be little question, therefore, of much coherence, even if the, often conservative, professors at the head of these societies were prepared to publish speculative hypotheses.⁵¹ Certainly the younger geologists, for whom the distinction between geophysics, geochemistry and geodesy no longer meant very much, felt that they needed an international journal in which people could publish freely about new insights into the tectonics of the earth's crust and mantle. To this generation belonged Arie Manten, born in 1933, who came to work at Elsevier in 1960, a man of enthusiasm, with a luxuriant curling beard.⁵² These are his words:

In 1955, during my geology studies in Utrecht, I did research into the old coral reefs on a Swedish island, and there was so much interesting material that Professor Martin Rutten, who sent me there, wanted me to do a PhD on it. But I put off writing the thesis. It wasn't completed until 1971, and the title was *Silurian Reefs of Gotland*. The organisms in these reefs appealed far less to my imagination than the hypothetical *missing link* in man's origins, the ape-man whose remains Raymond Dart from Johannesburg claimed to have discovered. That's why I went and did field work in Makapansgat and the Karroo of South Africa, but I found nothing of interest. Then, as I needed money I went to work at Elsevier, where I ended up more or less by coincidence. A good friend of my parents, who was an editor for the *Winkler Prins* encyclopaedia, had spoken to me enthusiastically about the small science publishing company of Elsevier, with a staff at the time of only 40. For a year long I learnt the tricks of the publishing trade. Then, in 1961 I wrote a report for the director, Piet Bergmans, about the possibilities for a geological list. At the time I was deeply impressed by all the new discoveries I had heard about at the big four-yearly international geological congress the previous year in Copenhagen. Didn't all this new material just scream for something new in communication? So I put forward the plan to compete with *all*

⁵¹ Interview of Arie Manten on 16 November 2005.

⁵² Letter from Otto ter Haar to the author of 15 November 2005: 'With his fine nose, energy and perseverance, Arie Manten succeeded in setting up a completely new, flourishing branch of geological journals. But he started so quickly with his acquisitions, and so vigorously, that Piet Bergmans feared that his ambitions were greater than his possibilities. In no time he had founded a dozen journals, and although they were not yet profitable he wanted many more. So Piet wanted a breather, whereas Arie with his knowledge of the riches of the geological field argued that the time was ripe for harvesting. With hindsight Arie was right, but Piet was right in his sense that this valuable, gifted young man was inclined to rush past himself.'

the geological journals of the science fellowships. Elsevier must publish at least a dozen journals, in which the boundaries between the old fields of study would be broken down and the earth sciences, as it were, redefined. I knew there was plenty of interest, and that no publisher had yet explored this market. Bergmans gave me his full cooperation, as he was keen to expand his market. Naturally I had to submit a proper cost and benefit budget for each journal, but I was able to seize any opportunity, and in the 1960s there were plenty of those for whoever entered that particular market. I started with *Sedimentology*, and went on to two new journals per year, and in 1967 managed to add four...⁵³

Now we shall restrict ourselves to the history of the journal *Tectonophysics* from 1964, where Arie Manten, from a scientific point of view, derived his greatest honour. Thanks to the way in which he set it up, it succeeded in attracting a significant amount of the discussion around the movement of continents, which also included a large international and multidisciplinary council of editors and a guaranteed fast publication. Manten negotiated that there should be no chief editor, so that it would not bear the stamp of any one person.⁵⁴ Articles would be sent to a member of the editorial team who was thoroughly familiar with the subject and could respond quickly, without the intervention of any other person.⁵⁵ To speed up the process he wanted it published in cold type and offset print, a method that was used for the first time for a primary journal at Elsevier (but which Van Tongeren had already used 10 years previously for secondary journals of *Excerpta Medica*). To do this it was essential that Elsevier had the editorial office under its own control, just as in the case of *Biochimica et Biophysica Acta*, so that they could keep a sharp eye on whether assessment of manuscripts by selected editors took up too much time. This supervision would be entrusted to a secretary, Anton Stoffels. And Manten wanted something else: because the journal must be of the highest scientific quality—otherwise it would be impossible to compete with the scientific institutes—it should set the standard! A consistent nomenclature should be used, which was laid down by an international committee for terms in the earth sciences, of which he was a member. So ‘wrong’ names in authors’ manuscripts

⁵³ Interview of Arie Manten on 16 November 2005.

⁵⁴ The Introduction to this journal (*Tectonophysics* **1** (1964) 1–2) is not signed but simply ends with a wish: ‘Both Editorial Board and publisher hope and trust that this new journal will contribute to a better understanding among people in the world of the geo-sciences.’

⁵⁵ Interview of Arie Manten on 16 November 2005.

had to be replaced in the editing office by the ‘correct’ ones. He was unable in the beginning to foresee the consequences in the kind of irritation, delay and cost this would bring.⁵⁶

Before this, however, Manten was able to bring together, in perfect harmony, 25 geologists from around the whole world to back his idea for a journal. In 1962 he developed this idea into a concept, and presented it to four people whom he had got to know during his fieldwork in Sweden and South Africa, and whom he now revisited.⁵⁷ They were Wieland Gevers, Jacob Hospers, Hans Ramberg and Nick Rast, from respectively Johannesburg, Amsterdam, Uppsala and Liverpool. Ramberg studied the flow of rock under high pressure, for which he carried out laboratory experiments, and because of this physical approach to structural geology—tectonics—he suggested the name *Tectonophysics* for the journal. This name could no longer be questioned by North-Holland because the gentlemen’s agreement between Frank and Bergmans to divide the market had just been terminated. Manten took over the name and, together with Gevers, Hospers, Ramberg and Rast, wrote to all key figures who were involved in discussion on the plates and the mantle. Keith Runcorn and Tuzo Wilson, who we have mentioned above, backed the journal, and also Vladimir Belousov, which was perhaps even more important. Besides this the Upper Mantle Committee, which had created the international union of geologists, promised to send on articles that might qualify for publication in the journal.

In spite of these excellent anchors in the scientific world, the journal was not immediately successful. In 1964 there was only sufficient copy for one volume of 578 pages, and the number of subscribers did not exceed 500. Bergmans followed this development with Argus’ eyes. He got the impression that there was no great demand for geological journals, while besides *Tectonophysics*, Manten had already launched eleven

⁵⁶ Letter from Otto ter Haar to the author of 15 November 2005: ‘Member of an international committee for the geological nomenclature, Arie Manten requested the newly adopted names in his journals. When the world’s learned societies had agreed to use them, Elsevier couldn’t stay behind, since we claimed to publish quality journals. Arie therefore instructed his desk editors to change the names if they were not correct, but he promptly became entangled in quarrels with authors, who requested that the changes be undone. By the delays and quarrels a number of his journals ran into problems, so much so that I had to intervene and force Arie to give up his policy of correcting names; also his own desk-editors and in some cases Boards of Editors had protested. This clearly was a case in which his thrust carried him too far.’

⁵⁷ Interview of Arie Manten on 16 November 2005.

more.⁵⁸ Before setting up new ones, he wanted to see whether the old ones could make money. Fortunately in 1968 *Tectonophysics* at last made a profit, because the two volumes they were able to publish—an enlarged copy that reflected increased scientific interest—had raised the number of subscribers to over the 500 mark. As an illustration of this increased interest we mention Tsuneji Rikitake's long article on earthquake prediction in the first issue of volume 6 from 1968. At the time Herman Frank (no relation to Frank of North-Holland), who had succeeded Stoffels, became chief editor, and then Manten unfortunately fell ill with serious heart problems.⁵⁹ He knew that it would be a long time before he fully recovered so, with Bergmans' agreement, he appointed to take over management of the journal a 28-year-old geologist, who succeeded in demonstrating that it could grow still further. Tuzo Wilson helped him out by informally taking on the role of mentor and chief editor. In 1984 the number of volumes would exceed 10 per year, while even sooner the number of subscribers would be over a 1000—a maximum for the geological community. We leave the introduction of this new manager of the geological list until the next chapter, where he will be frequently mentioned.

The histories of these journals cannot be concluded without saying something of the policy that united them.⁶⁰ In the 1960s Elsevier followed a policy of fast growth. The first thing that had to be seen to was that articles must be published quickly. For prompt publication attracted more authors. Therefore much was invested in an efficient

⁵⁸ In 1963 *Geoexploration* and *Journal of Hydrology* were started; in 1964 *Marine Geology* and *Tectonophysics*; in 1965 *Paleo* and *Photogrammetria*; in 1966 *Chemical Geology* and *Earth Science Reviews*; in 1967 *Engineering Geology*, *Geoderma*, *Review of Palaeobotany* and *Sedimentary Geology*; and in 1968 *Lithos*. The fore-last journal was to become the largest and had at a certain moment more than 2000 subscriptions.

⁵⁹ Letter from Arie Manten to the author of 23 November 2005: 'My illness—a toxic goitre which disordered the heart-beat—had nothing to do with my work; I had much to do, that is true, but I liked my work and was always ready to talk to Elsevier-clients in the evening hours or weekends, what others tried to avoid.'

⁶⁰ Letter from Otto ter Haar to the author of 27 January 2006; the present paragraph follows Ter Haar's wording of the journal-policy, but there have been other opinions—initially a great share in the market hadn't been goal number 1. Arie Manten writes in his letter to the author of 23 November 2005: 'In the period 1960–1968 the aims were quality and market-share, in that order. Willem Gaade wanted the highest possible quality and reliability, since he had to compete with renowned German publishers with chemical editions where one couldn't find a single error. He could blush up to his ears when he came across a negative review of one of Elsevier's editions. Piet Bergmans agreed with him—he used to listen carefully to the opinion of Elsevier's authors.'

organisation of the editorial assessment, the desk editing and publication. Once a journal was in demand, it was time to attend to the quality of the articles. The rejection rate was raised to such an extent that the honour of being published in the journal outweighed the disappointment of being rejected; high reputation attracted even more authors. If contributions decreased, the rejection rate could be lowered, but then the journal would be on its way down. This policy ('first speed—then quality') worked well for as long as the scientific societies hadn't solved the problem of lengthy publication times for their journals. For the smaller societies that did not publish a great deal the problem was unsolvable; and for the large ones it was also a problem, even if the authors were asked to pay for their publications. This is why much of what these societies used to publish themselves quite soon ended up with commercial publishers like Elsevier, where there was no page charge. The disadvantage of this growth policy was that it was difficult to predict how many pages a volume would contain. Since this usually turned out to be more than predicted, it often happened that a subscriber would be asked to pay for an addition to his subscription. During the boom of the 1960s this led at most to irritation, but in the 1970s these extra charges met with growing resistance.

The Integration

Those who wish to appreciate the sum and substance of science publishing around 1970 cannot do so without the details about the discoveries that lay behind it, which we have looked at already. But for those who wish to see it as a merger of companies—in this case the development of the Associated Scientific Publishers—should stand back and widen their horizons. This is what we shall now do.

Naturally our protagonists still combed the universities and institutes to see whether new profitable journals, or otherwise interesting book series, could be created: Wim Wimmers and Einar Fredriksson for North-Holland, Marc Atkins and the now fully recovered Arie Manten for Elsevier. The former pursued the physicists and economists 'till it made them drowsy';⁶¹ the latter chased after the chemists and the agronomists or ecologists—and the efforts of newcomers such as Pieter

⁶¹ Interview of Einar Fredriksson on 5 November 2003.

Bolman are not even considered here. But wherever they presented themselves the army of Academic, Blackwell, Pergamon, Springer, Wiley and all the other publishers had often got there first and made off with the pickings. Setting up new journals had never been difficult, and nor was it now, but setting up truly profitable ones in the 1970s proved no longer to be possible. The exception that seemed to prove the rule was *Trends in Biochemical Sciences* that was launched in January 1976 and sold like wildfire.⁶² But this was no 'ordinary' journal. It was a magazine along the lines of MacMillan's *Nature* and each month contained new developments and short summaries on biochemical research. The two streams that had originated separately in North-Holland and Elsevier now flowed as one in these *Trends*—the streams of *Biochimica et Biophysica Acta* and of *FEBS Letters*—and they symbolised the integration of these two publishing houses. If, however, we disregard this magazine then none of the 'ordinary' journals that Wimmers, Fredriksson, Atkins, Manten and the other newcomers added to the 88 of 1970 were either of any great size or particularly profitable.

We give some bare figures. In the catalogue of the Associated Scientific Publishers of 1975 we find 213 journals, 102 of Elsevier, 64 of North-Holland, and 47 of Excerpta Medica; the last were wrongly

⁶² The idea of the magazine goes back to Jack Franklin, a biologist working for Elsevier, who in 1973 started to argue that biochemists needed a kind of newspaper—see the article by W.C. (Bill) Whelan in *Trends in Biochemical Sciences* **9** (1989) 3–5. Whelan writes here about his inclination to become involved in publication activities, and that he had assisted in the founding of the *FEBS Letters* and thus knew Bart van Tongeren, i.e. North-Holland. On the other hand, his colleague on the board of the International Union of Biochemists, E.C. (Bill) Slater who was chief editor of *Biochimica et Biophysica Acta*, knew Jacques Remarque, i.e. Elsevier and its director Otto ter Haar: 'Since the two companies had recently merged I discussed with him [Slater] of meeting Elsevier/North-Holland to exchange ideas about new ventures. A meeting [with Van Tongeren and Ter Haar] took place in [the Commercial Club in] Amsterdam in December 1973. Among the ideas that I introduced was one of the kind of *Nature*, specifically aimed at biochemists, something of the style of a newspaper, with news, reviews and original communications. Elsevier [Ter Haar] told us of their idea for a magazine for biochemists, something after the style of a newspaper, not publishing original material, but otherwise sounding like our proposal—so much so that we would together work on this new venture.' It took one and a half year, however, before the scope of this magazine was fixed and the access to sources as well as the editorial checks were secured, with Bill Whelan in the U.S.A., Joan Morgan in the U.K. and Jack Franklin in the Netherlands. And still: 'A worried Van Tongeren asked me [Whelan] to go to Amsterdam in August 1975 to review with the Elsevier staff what material was available for first few issues. The countdown to January 1976 and the first 18 months thereafter were stressing. But we have evidently succeeded, as much by good luck as good judgment, in this experiment in publishing.'

included because *Excerpta Medica* was not part of the Associated Scientific Publishers. By far the most, 199, had less than five volumes per year. Of the fourteen with more than five volumes, eight belonged to Elsevier and six to North-Holland. In that year Elsevier published eight volumes of *Analytica Chimica Acta*, 41 of *Biochimica et Biophysica Acta*, eighteen of *Brain Research*, eight of *Clinica Chimica Acta*, fifteen of the *Journal of Chromatography*, seven of the *Journal of Molecular Structure*, eight of *Mutation Research*, and seven of *Tectonophysics*. In that year North-Holland published twelve volumes of the *FEBS Letters*, eight of *Nuclear Instruments and Methods*, 36 of *Nuclear Physics*, seven of *Physica*, twelve of *Physics Letters*, and nine of *Surface Science*. Most of these great journals have already been discussed.

The fact that we mention *Physica* here—the renowned journal in which most Dutch physicists published their findings—requires some explanation. We have already described how it was launched in 1934 by the publisher Martinus Nijhoff in The Hague, and taken over by a foundation in 1951. It so happened that in 1966 this foundation ran into financial difficulties, because the single volume that it published each year got larger and larger, reaching more than 2000 pages, while the subscription price was not adjusted in time commensurate with its greater size. Then Joost van den Handel, the professor from Leiden who edited *Physica*, asked Wim Wimmers to take it over.⁶³ It was a matter of course that he should ask Wim Wimmers because North-Holland was already in charge of production. Wimmers was perfectly happy to take over the journal, but didn't intend to put any more money into it. This is how, in 1967, North-Holland declared itself to be the publisher (although it would not be until 1985 that it took over ownership of the foundation), and in that year four volumes of 600 pages promptly appeared, with realistic subscription prices that went up by leaps and bounds. Incidentally, it was not in *Physica* that the Dutchman Gerard 't Hooft published the article in 1971 that would earn him a Nobel Prize, but in *Nuclear Physics*.⁶⁴ After the further growth of *Physica*, the journal was split up, in 1974, into three parts (A for statistical, theoretical and molecular physics; B for the physics of solid matter and low temperatures; and C for physics of plasmas and for spectroscopy and

⁶³ Interview of Wim Wimmers on 11 March 2003.

⁶⁴ G.'t Hooft, 'Renormalization of mass-less Yang-Mill fields', *Nuclear Physics* **B33** (1971) 173–199.

optics). So in the following year, when the catalogue we mentioned was published, it had seven volumes.

The value of these big journals for the enterprise—they hardly exceeded a dozen—was enormous. They provided as much as two thirds of the profits of the Elsevier holding, even though the turnover for Associated Scientific Publishers was only a quarter of that of Elsevier as a whole.⁶⁵ This proportion remained the same for a large part of the 1970s, and was already present in 1970, the first year of Associated Scientific Publishers. At that time they made a profit of 11.6 million guilders, against the 17.2 million profit made by the Elsevier holding company.⁶⁶ Nobody really thought that this exceptional state of affairs could continue. The factors that started the money flow, strengthened it or weakened it, were, after the boom of the 1960s, mostly uncertain. How would the international market, on which the company was largely dependent, develop? Who could have foreseen that the Bretton Woods Agreement on stable exchange rates would be cancelled and that the dollar, when the Vietnam War came to an end, would lose 25% of its value?⁶⁷ Would American competitors, who had enjoyed comparable flows of money, wish to use their growth to buy up publishers such as Elsevier in Europe? Such a possibility could never be altogether excluded, however carefully its share capital had been protected.⁶⁸ For it seemed improbable that the source of all this money would dry up. Governments still invested in scientific research—and they did this for strategic reasons, for the sake of national security and for greater welfare—but they were already shifting public spending in other directions. In 1975 West European countries spent around a third of their gross

⁶⁵ Letter from Bart van Tongeren to the author of 8 September 2004, where he quotes his contribution to a summer school in 1978 of the Publishing Institute in Denver (Colorado): ‘Elsevier has a turnover of about 260 million dollars, which is achieved by its five divisions (Trade books, Trade journals, Science books and journals, Printing and Retail). The turnover of the Science division is about 1/4th of the total turnover, but this division contributes about 2/3rd of the profit of the entire group.’

⁶⁶ Letter of Bart van Tongeren to the author of 24 July 2004, p. 16.

⁶⁷ Abel & Newlin pp. 204–205, where Hendrik Edelman sketches the ‘Doomsday’ at the end of 1971, brought forth by the drastic devaluation of the dollar, and its consequences for the trade in science books and periodicals; earlier in the book, on pp. 5–6, Albert Henderson writes about the prediction of ‘Doomsday’ by Derek de Solla Price and David Goodstein and about the modest set-back occurring in reality. [Factual take-overs of science publishing companies are discussed in Chapter 9.]

⁶⁸ Letter from Otto ter Haar to the author of 24 November 2005; no details are given of this protective construction, however, except that it was ‘nailed down’.

national income on social services, almost twice as much as in 1950.⁶⁹ And then: what of the effect of the returning unemployment after the third Israeli war and the oil crisis?

These were the worries of the men at the top, although no-one who watched them at the party at the home of Bart van Tongeren, on the occasion of the international publishers' congress in 1968, would have guessed as they enjoyed themselves to the hilt and Captain Robert Maxwell or *Herr Doktor* Konrad Springer (which of the two no one quite remembers) stood on his head to the enthusiastic applause of all present.⁷⁰ In any case, Otto ter Haar—who we have already mentioned—didn't let it worry him, for he had learnt to worry only about those things that one actually can do something about. This he had learnt during his interrupted medical studies, in the logistics of transportation of tobacco (or if necessary rubber, latex and palm oil) for the Deli Company from Medan on Sumatra to Amsterdam, for which he once on his own loaded an entire ship, and after that at Elsevier.⁷¹ This is how he describes those times:

In 1958 I ended up in Amsterdam, after nationalisation of the Deli Company by the Indonesians, and carried on working in the tobacco and rubber trade. But I couldn't bear the thought of a life spent amidst telephones and telex machines, deprived of every contact with the products and all the colourful characters that came with them. That's why I went out in search of other work. I found it at Elsevier's Science Publishing, where Piet Bergmans was looking for someone to come and work in promotion and distribution. His son Louk would teach me what this entailed. That was in 1959, when I was 30. What a change! From the princely accommodation on the Herengracht, appropriate to a wealthy colonial company, I suddenly found myself in a cramped office in the garrets of the Spuistraat, where the barely financially viable science department of Elsevier was housed. The informal, fairly relaxed working relations there,

⁶⁹ Schuyt & Taverne p. 305.

⁷⁰ Letter from Bart van Tongeren to the author of 31 October 2006; see also Chapter 9.

⁷¹ Interview of Otto ter Haar on 11 April 2003: 'As a young lad of 18 I wanted to become doctor and thus started a study of medicine in Leiden. That was in 1947. It took some time to discover that a medical profession was nothing for me, but then I took the helm and headed for the Deli Company in Medan (Indonesia). There I worked from 1951 to 1956 with quickly growing responsibilities in the logistics of transports to Amsterdam, and after this first contract period I worked another two years for Deli in Jakarta. In 1958, however, when many countrymen were leaving Indonesia (which until 1949 was a Dutch colony) I found it wise to return to Holland as well. Back in Holland (in Amsterdam) I soon found my way to Elsevier.'

though, were like a breath of fresh air after the old-fashioned, hierarchical and almost military discipline of a post-colonial concern.

At Elsevier there was barely talk, as yet, of specialisation. Already as a newcomer I sat in on the discussions about their publications, and learned what acquisition entailed, the desk editing, production and the promotion and sales that were to be my responsibilities. After a couple of years they put me onto the catalogue. Editors delivered pieces of text that I had to streamline and tidy up. This way I got to know about the publications and the people behind them. Of course, they had to be turned out perfectly, and anyone who wants something to be really good must possess certain diplomatic qualities... The organisation of this catalogue fascinated me, but the organisation of the publishing company as a whole fascinated me even more. During the 1960s this had to be constantly adapted to the prevailing growth, because it just couldn't carry on being so simple and informal. I made my career quite quickly only thanks to the fact that I was able to be the oil in the works: I quickly saw when things came to a standstill and knew what to do about it. After I had learnt about the company in all its facets, I succeeded Piet Bergmans. That was in 1972. Piet, the hard-working pioneer from its first hours, had become a widower and, after the amalgamation with Nohum [North-Holland], wished to start a new life.

The amalgamation was a long process. At Nohum, a proud and very personally led publishing house, people were frustrated by the fact of being taken over by Elsevier, their old competitor; although everyone staunchly avowed that it was an amalgamation. We [at Elsevier] were already accustomed to changes, even if these were only periodic interventions within the holding, but we feared the burdens, as well as the joys, that this marriage would bring. So the matrimony was consummated with great caution. It was in the interest of Bart van Tongeren as well as myself that the two company managements be joined together on an equal basis, but then in proper consultation, so without haste and without friction. In this Chris Baltzer, the sales manager at Nohum, played an important part. With Germanic thoroughness and endless energy he weaved the sales of Elsevier and Nohum into one another, which quickly ensured that ASP [Associated Scientific Publishers] got a face of its own. No one could foresee at the time that Bart and Chris would leave after a couple of years. In 1978 I was on my own.⁷²

When we asked him to say something more about himself, he added:⁷³ Organisation and improvisation, those are the key words. Organisa-

⁷² Letter from Otto ter Haar to the author of 30 January 2006.

⁷³ Letter from Otto ter Haar to the author of 26 February 2006 (written upon telephonic request to elaborate on points in the above letter—Otto lives in France): 'Current Contents', so important for us, was one of Garfield's products, like the *Citation Index*. Cunning and with fascinating ideas, but also extremely thrifty, Garfield paid his

tion—that didn't surprise us. What did surprise us was improvisation, and we asked him to explain. He shouldn't be told what to do, he said; he needed to find out for himself. If he hadn't arrived at Elsevier in 1959, but into the controlled, specialised and compartmentalised company that it is now, then he would not have lasted for long. 'Don't ask me what I'd do now.' Bergmans gave him all the freedom he needed. He found it hard *not* to talk about Bergmans, the ideal manager, the aesthete who, after three or four years, gave him the task of making the company's new catalogue—what in hindsight was the baptism by fire that he must go through in order to grow, and which after weeks of working into the night with Helmut Salden, the typographer, resulted in a publication reckoned amongst the best produced books of the country.⁷⁴ Again, it was Bergmans who sent him off in his Citroën Deux-chevaux full of folders, book volumes and journals to call on the bookshops of Europe, before letting him go further afield, to clients in faraway countries such as India, Japan and China. Here, indeed, it came down to improvisation, and when he described it all it was just as if we saw before us the trader in colonial wares. In India there were the dark lairs where clerks sat typing orders on ancient machines, and the bosses most probably kept up corrupt relations with the librarian of the local university... In Japan where no one yet spoke any English and on a first visit you could only exchange words on the weather through an interpreter, and on one's edifying impressions of the country, after which a discussion might follow with professors who understood a little German... And in China, or rather in the state offices of Beijing, where the heating was turned off on 1 March, and you were received in the bitter cold by people who looked like bales of fabric, in woolly hats that barely showed their eyes...

Those who have spoken with him, this lively Otto ter Haar with his twinkling eyes and distinguished moustache, have little trouble in imagining how he once sat with Remarque or Gaade or Atkins at their editing desks—veritable enterprises within an enterprise—at meetings with their editorial boards, and how he picked the brains of professors

people so little that his company had a strongly floating staff that was less and less competent. He was regularly invited to be guest in Vinken's manor house. Vinken hoped to buy one day his company [ISI], but although Garfield had promised that Elsevier would be his first choice, he finally sold it to someone else—much to the Vinken's dismay.'

⁷⁴ Letter from Otto ter Haar to the author of 30 January 2006.

in his high voice on the secrets of scientific communication! ‘Because they kept coming up with *Current Contents* meant that we had to send our journals to Garfield’s institute, just like that, for nothing, hoping and praying that the contents would be published in his book.’⁷⁵ He should have studied anthropology, this university drop-out, not medicine, because his insights into people and situations were astonishing. By deepening his knowledge of the personal backgrounds of his staff, he found out what made them tick, and how to harness their strengths. And as far as their weaknesses were concerned—he had plenty to tell, but not a word about these should be written down.

But we were not going to discuss details anymore, and shall therefore say just a few words about the manoeuvres of the bosses, who usually get embroiled in conflict when there is a merger. It has already been mentioned that the Elsevier holding would get an executive board. Bart van Tongeren would manage science publishing, Jan Verleur finances, and Pierre Vinken the development of information systems. Naturally Dolf van den Brink, the man who had led the company almost single-handedly for more than twenty years, would become chairman. In addition, he took care of the book trade, the branch that had always been closest to his heart. Even a close friend acknowledged that he could at times be ‘authoritarian, arrogant and impatient’.⁷⁶ Bart, who had his own ideas about Dolf, didn’t much relish sharing with him managerial responsibilities, and stayed on for a time at North-Holland on the Keizersgracht. Guus Zoutendijk ensured that any disputes that arose between Bart and Piet Bergmans (and after 1972, Otto ter Haar) were speedily resolved, thanks to his business-like chairmanship; and not always to the disadvantage of the former. ‘Taking into consideration the great amount of work attached to the integration of the ASP companies, 1 January 1973 would seem a good date for entry into the board of the holding’,⁷⁷ but Bart needed the time for an ‘inner adjustment’. The time came, and he moved over, together with his 80 members of staff, to the Jan van Galenstraat, a considerable distance from the city centre, on the western ring road. Ten years earlier new

⁷⁵ Letter from Otto ter Haar to the author of 26 February 2006.

⁷⁶ Brink (*De uitgever*) p. 10 (from the Preface by Floris Bakels).

⁷⁷ Memorandum *Hoofddirectie UME* (Central Management Elsevier’s Publishing Company) addressed to Dolf van den Brink, written by Bart van Tongeren and Pierre Vinken and dated 12 July 1971.

premises had been built there for Elsevier—an office with facings made of natural stone and coffin-shaped windows, which already shows that it was something unusual. The world-famous *Bauhaus* architect Marcel Breuer had designed it, and Prince Bernhard had wanted to open it, together with Max Perutz, a Nobel Prize winner who was so kind as to allow Willem Gaade to publish his book on *Proteins and Nucleic Acids* for the occasion.⁷⁸ A wing had been added for the merger, so that staff from North-Holland and the science publishing department of Elsevier could physically walk from the one to the other. In fact, the wing was so spaciouly built that the entire Elsevier company could be accommodated, and the office on the Spuistraat could be vacated. The top floor consisted of a vast library, in which for years the shareholders held their meetings, and an even larger empty space, which could be used for exhibitions. It was in these new premises that Bart suffered health problems a few years later, as he explains in the following:

My load was too heavy, and then I got backache. It reached a stage when I could only walk with the help of a stick. My direct management of North-Holland, which I'd been doing for two years to ensure a smooth transition for my own staff, had come to an end. But after 1975 the Associated Scientific Publishers demanded a lot of my time, as I had to take matters in hand at the New York office. Not only there, but I also had the development of local lists at Barking in England, and in Lausanne, Mexico and Tokyo to coordinate, so I was already travelling a lot anyway. Business had come to a standstill in New York due to mismanagement on the part of Ben Russak, our caretaker manager there. When Paul Houber, a thoroughly nice man who I put there instead made a total mess of the publishing, I saw no other alternative than to take over the management myself for several months. That meant a board meeting on the Monday in Amsterdam, then the rest of the week reviving the staff in New York. I may always have enjoyed travelling, but something like that is just exhausting, and it only came to an end when James Kels, who worked under Otto ter Haar, was prepared to go and manage the New York branch.⁷⁹

⁷⁸ Interview of Otto ter Haar on 14 May 2003: 'For long years Elsevier had had no decent housing, since Dolf van den Brink was thrifty, but when he finally decided to give us a new building—the one that was built at the Jan van Galenstraat—he wished a certain distinction and chose a famous architect. At the opening [in 1963] he wanted to get as much publicity as possible, and thus invited a member of the Royal Family, a member of the Nobel Committee and a Nobel Laureate. For a scientist only the last one will count. It was Max Perutz, who had discovered something in the blood and just had got the Prize.'

⁷⁹ Letter from Bart van Tongeren to the author of 2 January 2005 (paragraph 9).

But this couldn't explain the stick. His lameness was undoubtedly psychosomatic and the result of conflicts with Dolf, who he saw every Monday; every Monday he was again faced with the man's conceit and the way in which he addressed his board as if they were a class of school children, reading them the lesson on newly published books that he had read after mass the Sunday before—lessons Bart couldn't stop talking about, even after thirty years. Once when they were discussing long-term planning, he challenged the master to first produce such a plan for his own book division. Promptly Dolf conjured up the required 20% profit, but failed to come up with realistic figures on turnover and cost cutting. Did the man understand the slyness of the exercise? Then it was Bart's turn.⁸⁰ Together with Otto ter Haar and the accountant Tim Pöttker, he analysed 'realistically' the science list, and showed how easy it was to come up with the required profit: even if the number of subscriptions on the journals went down, the subscription price went up, and a couple of new ones were created. But it was an ineffectual way of saying that all these calculations really meant nothing at all, and the time came when this feeling of powerlessness went to his back. For good, it seemed. Pierre, the friend who had once been a doctor, declared him unable to work, and in 1977 Bart took early retirement, while retaining his salary. And as far as the other members of the board were concerned, the bright but somewhat gruff treasurer Jan Verleur couldn't hope to compete with the courteous and highly presentable 'professor' Vinken.⁸¹ No one was therefore surprised when the latter was appointed as successor to the president-director in 1978.

More interesting than the clash of personalities were the stakes involved. What were the profits used for? As we have already said, two thirds came from Associated Scientific Publishers. Would it not therefore have been a sensible idea to invest this in scientific publications? Had this question been asked during those Monday meetings, then undoubtedly Dolf would have answered that the flow of capital here is not necessarily greater than in the newspaper world—just to quote an example. Nor would Jan Verleur, 'who was exceptionally clever at devising tax constructions that deposited any risk firmly at the door of the state',⁸² have been a great advocate of this either. But it was Dolf,

⁸⁰ Idem (paragraph 6).

⁸¹ Vermeulen & Wit pp. 38, 49.

⁸² Letter from Otto ter Haar to the author of 17 November 2005.

and no other, who provided a destination for the money made from the journals—and to a lesser extent from the *Winkler Prins* encyclopaedia and *Elsevier's Weekblad*. In his opinion, in order to establish a strong position in the international market, it was necessary to obtain sales channels, and companies in other sectors, so as to reduce sensitivity to economic fluctuations. And under the motto 'I improvise best when I negotiate alone'⁸³ he bought up a number of companies during the 1970s that had little or nothing to do with international science publishing.⁸⁴ He began with Misset, who published, amongst other things, *De Boerderij*, by far the largest agrarian professional journal in the Netherlands, but which also included a printing works—a capital-intensive feature that meant it made little profit. We give only the names of all the other publishing companies: De Boekerij, Manteau, Stok and Van Goor (Dutch publishing firms that had already been brought together under Edicom), as well as Focus and PBNA, also Dutch; Phaidon Press in Oxford and the Ingenieur Digest in Frankfurt, Dutton and Medical Examination in New York; and from the large Spanish-speaking territories, Selecciones Editoriales in Barcelona and Libreria Internacional in Mexico that published medical works. Dolf also bought up bookshops: Coebergh, Dekker & Nordemann, Het Nieuwe Boekhuis, Hugo Jonkers, Marsman, Mosmans, Scholtens, Schoth, Van den Broek, Van Leeuwen, Voorhoeve and Van Stockum. Here we add that the investments in all these bookshops paid off in the beginning, but eventually had to be written off as a loss. He wanted to buy Nijhoff and Brill, too, but didn't manage to. And nor did he manage, the summer before his departure, to purchase two national dailies, *NRC Handelsblad* and *Algemeen Dagblad*—something which, incidentally, the executive board and the board of directors had authorized him to do. Or perhaps we should say he partially managed to do so. When he left in 1978, he left Elsevier with the legacy of a merger with Nederlandse Dagblad Unie, the publisher of both these newspapers.⁸⁵ Willem Pluygers, the

⁸³ Brink (*De uitgever*) p. 11 (from the Preface by Floris Bakels).

⁸⁴ Lente (*Nederlandse uitgevers*) p. 184; see also *Appendix* in Bakels' unfinished typescript (note 2 of the Preface of this book).

⁸⁵ Vermeulen & Wit p. 38; letter from Otto ter Haar to the author of 2 February 2006: 'We spoke of the geriatric contract. Both Dolf van den Brink and Willem Pluygers, the pope of the Dutch Newspaper Union NDU, were almost 65 and both claimed to have made the first move. Pluygers' background was in printing works (he was said to become hot when he smelled toluene) but we didn't like printing works because of their meagre profit, whereas newspapers weren't very profitable either. But Van den Brink

director, did not wish to be bought up, but to amalgamate on a basis of equality—an equality that did not, in fact, exist. Nevertheless, Van den Brink must have seen this amalgamation as his crowning glory.

Those interested only in science publishing must wonder what kind of purchase in the 1970s could have contributed to Associated Scientific Publishers. To answer this we need to look at the possibly under-developed physics list and focus our attention on plasma physics, a hot topic of the 1970s. Plasma physics was important in understanding the essential processes in stars and interstellar material, but also in attempts to bring about controlled nuclear fusion. In 1960 Vitaly Ginzburg had published a very good book on the subject in Moscow, which was immediately translated into English and appeared under the title *The Propagation of Electromagnetic Waves in Plasmas*. Wim Wimmers had bought the rights for North-Holland from Gordon & Breach in New York, publishers of the English translation, for exclusive distribution outside the Western Hemisphere.⁸⁶ There was plenty of demand for the book, but when a reprint was needed and Ginzburg also wished for improvements, Maxwell pounced. So the beautifully produced second edition was published by Pergamon, which needed no other publisher to distribute the book over the whole world.

Meanwhile, in 1967 Wimmers did manage to publish a jewel of a book, *Theoretical Methods in Plasma Physics*, by Nico van Kampen and Ubbo Felderhof from Utrecht. Concise as it was, it was unable to replace ‘the thick Ginzburg’, so was never reprinted. The topicality of the subject was again emphasised in 1970 with a Nobel Prize for Hannes Alfvén ‘for fundamental work and discoveries in magneto-hydrodynamics, with fruitful application in various disciplines of plasma physics.’ Because Gaade had once arranged that he should have sole right to publish the addresses at Nobel Prize award ceremonies—a

wanted to become the largest publishing company in the Netherlands, and convinced his colleagues on the board that a purchase of NDU was advantageous for the company. It was in fact Elsevier that bought NDU, but out of respect for *NRC Handelsblad* the deal was called a fusion. Moreover, the charming, but autocratic and unscrupulous Puygers quickly prevailed in the management of Elsevier-NDU. He was like an oak under which nothing can grow. After having profited from the large common capital and an attempt to buy further newspapers, Elsevier sold NDU again in 1995.’

⁸⁶ Interview of Wim Wimmers on 11 March 2003.

costly privilege⁸⁷—Elsevier did publish Alfvén's explanation in 1972, but someone else went off with the profits. It was Reidel who published the detailed basic text in 1975: *Structure and Evolutionary History of the Solar System*, with the master himself and Gustaf Arrhenius as authors. This book clarified how electro-magnetic Lorentz forces and the plasma waves named after Alfvén must have led to and controlled the aggregation of material around a heavy body such as the sun (or a planet), and resulted in much discussion in the 1970s. But where was North-Holland all this time?

North-Holland no longer had a name in this field, in spite of the large number of physicists who worked there, and even though their number was increasing! The fact is that plasma physical processes are related to processes in magnetic material and semi-conductors, a field of considerable industrial interest.⁸⁸ For, however strange it may sound, electrons can behave almost freely in material ('solid matter'), just as in extremely hot gas (a 'plasma') and, thanks to their magnetic moment, are able to interact with each other. Their interaction can cause chaos as well as well-ordered phenomena, such as superconductivity. It is no coincidence that Alfvén had to share the 1970 Nobel Prize with Louis Néel, the Frenchman who was awarded his part 'for pioneering studies of the magnetic properties of solids'. (Science philosophers need to explain how the 'non-linear' physics of chaos and superconductivity could be discovered, because a paradigm shift à la Kuhn, or a falsification à la Popper, would seem inappropriate.⁸⁹) Wimmers realized that here was something new, otherwise in 1974 *Physica* would not have

⁸⁷ The secretary of the Nobel Foundation, Niels Stahle, had given Elsevier the right to re-edit all Nobel Lectures in all five categories which had been published since 1901, with little care and little regularity, however. They started as simple speeches at the presentation of the Prize but became sophisticated lectures. Willem Gaade traced the scattered and often damaged old editions, reconstructed the texts, and involved Helmut Salden, the best typographer he knew, as well as the best printer and binder in the Netherlands. The first book of the beautiful series was offered to the aged King Gustaf VI, in a specially arranged audience in his Stockholm palace, and from then on Elsevier was invited at the ceremonies where the Prizes were presented. For Elsevier, the books had only propagandistic value, and in 1970 the costly series with few subscriptions was terminated. (Facts according to Otto ter Haar—see also note 46.)

⁸⁸ Nye pp. 413–428 (Chapter by Michael Eckert).

⁸⁹ Nye p. 427: 'The time is not yet ripe for a historical appraisal of how nonlinear physics has restructured solid-state and plasma physics during the last decades of the twentieth century. However, [...] it once more brings to the fore some doubts about the established views on the growth of science. Usually, disciplinary or sub-disciplinary growth is considered a process of specialization, but plasma and solid-state physics acquired their identities by integration.'

given it a separate section, *C*. But this is why it is so remarkable that the series of monographs started up by the magnetism expert Peter Wohlfarth for North-Holland in 1974 no longer appeared in the catalogue of 1975.

Too little success in the field of plasma physics, in which almost half of the physicists were working? When this question arose we tried to discuss it with Wimmers, but sadly the 86-year-old was no longer able to speak to us. Another question—did no one see at Elsevier that Wimmers might need support? Otto ter Haar replied to this that ‘he would immediately have been given anything that he asked for, but that he was constantly away on his travels and never opened his mouth.’⁹⁰ If this is true—and the appointment of Pieter Bolman shows that the board of directors at Elsevier did recognize the importance of ‘his’ list—then apparently it did not occur to Wimmers that a publishing company could be purchased. They could have penetrated the plasma physics market via Reidel, a company the size of Misset, and the growing importance of magnetic materials and semiconductors would certainly have justified such a purchase. It could be, however, that *Physica C* wasn’t such a great success and that Wimmers simply left this field to the Americans, namely to the *Physical Review*. Bolman said that the plan was there to bring *Physica* up to the level of *Physical Review*, but that unfortunately he and Wimmers did not manage to carry it out.⁹¹

It is not for us to judge. We write only on what we understand. For example, on the fact that on his departure in 1978 Van den Brink felt the need to explain why all along he had been the man of ‘hear all, see all, say nowt’.⁹² This Rudolph Engelbert Marie was just scared stiff of taking risks. Or the fact that Baltzer, who had been in charge of sales for seven years at North-Holland and then for another five at Associated Scientific Publishers, didn’t wait long after Van Tongeren’s departure before handing in his own resignation. We realize that Johann Christoph wished to try his luck elsewhere. He went straight to the competitors. The somewhat shocked new head already had the

⁹⁰ Letter from Otto ter Haar to the author of 2 February 2006.

⁹¹ Interview of Pieter Bolman on 11 September 2003: ‘*Physica C* on superconductivity never came from the ground, and although we started *Physica D* on nonlinear phenomena and *Physica E* on nanostructures, the journal as a whole lost each year 5% of its subscriptions.’

⁹² Brink (*De uitgever*) p. 13.

impression that for some time he had not been entirely happy.⁹³ The North-Holland archives, which had been stored in the cellars of the premises on the Jan van Galenstraat, were already under water. Which brings us to the fact that in 1978 Floris Bakels, the executive secretary, was requested by the new head to stop with the jubilee book on the publishing company—in 1980 Elsevier would have been in existence for one hundred years. Not a soul would read it, would they, Pierre Jacques Vinken?⁹⁴

⁹³ Letter from Pierre Vinken to Chris Baltzer of 20 October 1977: 'I was shocked by your request to be discharged as ASP-director. The last time you weren't particularly happy in the top—that was my impression—but I did not expect that you wanted to leave already now.'

⁹⁴ Both Pierre Vinken and Willem Pluygers argued that it was not fitting to publish *Elsevier 100* when in the jubilee year 1980 Elsevier as such didn't exist anymore: Hadn't the fusion with the Dutch Newspaper Union the company changed in Elsevier-NDU? (See note 85).

CHAPTER EIGHT

SATURATION

Now that this history is almost complete, again we may ask: what makes it so interesting and why should people wish to read it? In the previous chapter science was discussed only indirectly. It dealt largely with people's behaviour and the tricks they play to achieve their goals, which seemingly has little to do with science—tricks that are the same the world over: 'eadem, sed aliter' (the same, but in a different manner). Or is this behaviour perhaps significant, after all? In other words, does a social history of scientific publishing have something fundamental to tell us about science?

In the 1970s, the period under discussion, people certainly thought so; the manner by which scientists came by new knowledge, not science itself, was to be the subject of historical study.¹ Naturally this included the way in which they imparted this new knowledge to one another, and the discussions this entailed. Their careers were studied, the organisation of their laboratories and their institutes, their journals, and their lobbies to finance 'big science'.² In this way the ideological character of science was 'discovered', and turned out to be not only a part of intellectual culture, but also of the entire apparatus of economic production. Science—natural science, at least—could not in any way be seen as distinct from a material culture. Its very essence lay, it seemed, in the *material*.³ It can surely be no coincidence that the greatest discovery of the 1970s was the quark, the fundamental particle that defines the nature of *matter*.⁴

¹ Olby & al. pp. 60–73 (Chapter by Barry Barnes) and pp. 87–99 (Chapter by Trevor Pinch); Krige & Pestre pp. 27–41 (Chapter by Simon Schafer).

² James Capshaw & Karen Rader, 'Big Science—Price to the Present', *Osiris* 7 (1992) 3–25.

³ Galison pp. 7–14.

⁴ Kragh pp. 322–324 and 342–344: 'Those hypothetical quarks—were they more than a useful mnemonic aid? They worked finely on the phenomenological level, but to many physicists they were merely simplistic expressions of the dynamics of a not-yet-understood world of hadrons. However, by the summer of 1974 the neutral currents [in these hadron dynamics] were firmly established. This amounted to a confirmation of the theory of weak interactions that, via quarks, had an obvious

This meant that the *ideas* held by scientists throughout the world remained largely uncommented upon. While everyone was fully aware that historians of music could not confine themselves simply to a description of the organisation of orchestras, people desired from historians of science only accounts of a series of occurrences, without any underlying pattern of *ideas* that might be discovered behind these occurrences. We needed an Imre Lakatos to exclaim that a history of science without the philosophy of science is blind.⁵ The synthetic unity of perception, which good old Immanuel Kant needed as a basis for theories about the world, could not possibly be dead.⁶ Nevertheless, Thomas Kuhn was cautious when he went in search of an idea:

History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed.⁷

Step by step he showed that the past was not a heroic picture of wonderfully great minds at work all on their own to unearth laws of nature just like that, as if they were truffles. Kuhn gave us instead the paradigm, a traditional, socially determined 'way of seeing' from which these great minds had to free themselves before they were able to discover these laws. This idea set many people thinking, but it also created much confusion. Why should traditional manners of thinking stand in the way of discovering something new? The paradigm had too much to do with habits, and too little with the world that must be explained.

What should we put in its place? We suggest that the history of science is a history of concepts—not of discoveries or of theories, but of concepts. We need concepts, as Georges Canguilhem and others have argued, because otherwise observations are not to be interpreted as discoveries, and no theories can be posited to explain them.⁸ Not having any scientific status, concepts give us the desired synthetic unity of

relation with strong interactions [between hadrons, and thus leant credibility to the real existence of quarks].'

⁵ Lakatos p. 102.

⁶ Cassirer p. 193 (quoting from Kant's *Critique of Pure Reason*): 'But because every appearance contains a manifold, so that different perceptions are encountered dispersed and singly in the mind, a connection between them is necessary, which they cannot have in sensation itself.'

⁷ Kuhn p. 1.

⁸ Olby & al. pp. 133–141 (Chapter by Gary Gutting).

perception. They provide an impetus towards understanding and must therefore be new, ‘novum et ad hunc diem non auditum’ (something new that people have not yet heard of).

Well then, these are the concepts that lie at the basis of science during the last century: the quantum and the gene. Recent books on the development of science even take their titles from them: *Quantum Generations*⁹ and *The Century of the Gene*.¹⁰ Each concept has its own history: the quantum dates from 1900, when it was postulated that the observable properties of atoms (and smaller particles) could not have just any value, but only discrete values—quanta. After it had given the impetus towards a quantum theory, in which the wave nature of atoms was somewhat naively included, quantum mechanics emerged in the 1920s and succeeded in describing phenomena in the world of particles (including electrons, the carriers of our communication technology) with amazing precision, followed by quantum field theory for electromagnetism and the forces between the elementary particles. But even more so, all physics of the century, and also chemistry, is an expression of the quantum. And as far as the gene is concerned—without this concept we would never have learned to understand how properties of living beings can be preserved, while the beings themselves are transitory (not preserved). In the 1940s, when we already knew how to determine the structure of molecules, the notion existed that inherited properties are coded in a protein structure. The discovery of this structure, the double helix in DNA, was then only a question of time. However, how the code (in a special sequence of chemical groups on the helices) is transmitted, only became clear in the 1970s when enzymes were discovered that could copy DNA into just such a molecule, namely RNA, which moreover can be ‘cut’ in pieces and ‘stuck’ together again. (What this has come to mean for medical science needs no further explanation.)

Are we forgetting other concepts that have directed the development of natural science in the past century? No, because the information-concept, which might qualify, is too abstract, too *immaterial* to be able to say anything characteristic about *material* nature,¹¹ and a series of other

⁹ Kragh *passim*.

¹⁰ Keller *passim*.

¹¹ Olby & al. pp. 538–540 (in the Chapter by Michael Mahoney): ‘Starting from the principle that information resolves uncertainty, Shannon measured information with reference to the number of possible messages that could be sent in a given time using a given set of symbols.’ Isn’t this concept far remote from physical things, i.e. things one can see, touch, or smell?

concepts had already become superseded ('empty space', 'ether' ...), or were no longer new (energy, element, evolution ...). The concepts that feature in a particular period in time are the concepts that preoccupy people. And in the previous century these were the quantum and the gene, and only these two.

Those who see the history of science as a history of concepts will, therefore, not be surprised at the fact that at times these concepts seem to falter or to become saturated. The process of observation (or experimental discovery) and theorisation that may come into force through a particular concept—this process will terminate when the concept has lost its strength. Previous concepts have also lost their strength. They are like the baker's yeast that causes dough to rise only so far, until no more carbon dioxide can be released. This means that knowledge of nature, and therefore profitable technological developments, will not necessarily increase if more means are made available for research. It also means that the demand for this knowledge will not necessarily increase if more professional journals are launched onto the market.

Some support for the above argument is to be found in the statistics on the number of people who work in science and technology, or to be more precise, the part of the population whose profession is to be found in these fields. According to Derek de Solla Price, who in 1963 was the first to write about this, in 1940 the percentage in the United States was around 0.3%, and in 1960 somewhere between 0.4% and 0.5%. It needs to be taken into consideration here that 0.45% is as much as 50% more than 0.3%.¹² In 1930, the year in which our history begins, it would have lain at around 0.2%. According to Susan Cozzens, who in the meantime has reviewed this growth and added new figures, in 1970 this was almost 0.6%, which doesn't yet indicate a lower growth rate; subsequently this growth slackened as in 1990 the percentage was 0.7%, or slightly above.¹³ For countries in Western Europe the figures are lower: in 1970 all are around 0.3%, and in 1990 it is almost 0.4% for England, 0.5% for France, and just under 0.6% for Germany—but for various reasons Germany forms a case apart. For this growth, and for the saturation that became visible during the 1980s, complex and inextricably intertwined political, economic and social causes have been proposed. Only seldom has it been suggested that the variations

¹² Price p. 12; the data are derived from Fig. 3.

¹³ Krige & Pestre p. 136 (Chapter by Susan E. Cozzens).

in this growth should not be attributed to such external causes, but to the one internal cause that we mentioned above—that periodically concepts cease to be fruitful, which is inherent in the practice of science. Why does this happen so rarely when great economists such as John Kendrick, Edward Denison and the Nobel Prize winner Robert Solow have provided convincing proof that new technology—possible as a result of new scientific knowledge—should be regarded as a fourth, and certainly not the smallest, production factor besides labour, capital and natural resources?¹⁴

Now we have come to the point when jonker Frans needs to be introduced. A sturdy figure, as cordial as he is discreet, who conceals any class consciousness beneath an air of easy informality. His full name, jonkheer Frans Willem Burmania van Humalda van Eysinga, is indication of his noble heritage, which for many centuries has been part of Frisian history. Not that he was actually born there; he is from The Hague. In 1978, at 38 years old, he entered the publishing house of Martinus Nijhoff to revive their ailing English department.¹⁵ His story provides an answer to the question that we asked at the beginning of this chapter, namely, whether a social history of a science publisher can actually tell us something about science. It will tell us of the saturation of the science market.

Frans came to work for Elsevier in 1968, after taking his time to complete his studies in geology at Leiden. He is the man who we introduced in the previous chapter, who had to take the place of Arie Manten when the latter became seriously ill. Of the many journals that Manten had put onto the market but which had not yet achieved success, *Tectonophysics* was the most interesting. At a congress for geologists in Moscow, shortly after his appointment, Frans met up with Tuzo Wilson, the Canadian editor of this journal—‘a man you could pick

¹⁴ Samuelson & Nordhaus pp. 534–535.

¹⁵ Interviews of Frans van Eysinga on 7 November 2003, 5 October 2004, and 26 June 2006. The first interview was devoted to Frans’ career, the second to the Nijhoff-history in so far as he knew it and the third to his expansion of the series of Kluwer-journals. His first remark on Martinus Nijhoff was: ‘When I came Hartger Hartgerink was already three years away. I was told that the man sometimes came along for a chat in the Nijhoff-bookshop, but he never came upstairs to see me—I cannot even tell you how he looked. I also heard that the former Nijhoff-directors used to be shaved by a barber, who every day came to the shop. The feudal nature of the house was notorious.’

out of a crowd any time'. They promptly discussed how the journal could attract more readers. First of all, they should publish in it all the contributions at congresses on plate tectonics! Such congress reports did indeed bring in more subscriptions, and raised the value of the journal—even if there were some deficiencies in the peer reviewing (here it should be noted that a publisher will always deny that in such a case the peer reviewing left something to be desired). It also helped that they had review articles written for the journal. But what helped most was that Frans would not be daunted. If a plan of his was rejected, then invariably he would come back with it, ever good-humoured and always with a smile, explaining that he had an entirely new plan or an entirely new idea. This got him a long way, and it would be this characteristic that would make him the ideal man to make something big out of something small.

Chris Baltzer was well acquainted with this characteristic—of Frans' amiable persistence—when he turned his back on Elsevier in 1978 to become director of Kluwer Academic Publishers. This was the name that he, Chris, thought up for the three publishers that were bought up by Kluwer in the 1970s. Kluwer, a small family firm in Deventer that published all sorts of things, began even earlier, in the 1960s, to buy up other family businesses, including Stenfort Kroese in Leiden (publisher of economics texts) and Tjeenk Willink in Zwolle (publisher of legal texts).¹⁶ The money for these purchases came from an entrepreneur who had made a fortune in the tropics. Rienk Visser, who was director of Kluwer in the 1970s, had to keep a sharp eye on the proceeds of this money, which caused him to make the ironic remark that 'in the last analysis, publishing is about turning printed paper into money.'¹⁷ Impressed with the profits Elsevier managed to make out of their science journals, he was prepared to pay a great deal for the couple of Dutch publishing houses that, alongside Elsevier, were also involved in the field of science. In 1970 he bought up Martinus Nijhoff,¹⁸ in 1972 Dr W. Junk, and in 1976 D. Reidel. We hand over to Frans:

¹⁶ Vries pp. 176–182; Lente (*Kluwer*) passim.

¹⁷ Vries p. 163: 'Rienk Visser emphasized that good publishing is primarily the art of making good choices, but it is also a higher form of packaging, namely the marketing of ideas: In the last analysis, publishing is about turning printed paper into money.'

¹⁸ Interview of Wim Koops on 28 April 2004: 'I was asked to sell the company on behalf of the shareholders, that is: the Nijhoff family. At the time I was commissioner of the company, but my main task was in Groningen, where I was librarian of the

Rienk Visser thought: if Elsevier can do the trick, so can I... He'd entrusted the supervision of these acquisitions to Frits Stenfort Kroese, the man who'd sold his firm to Kluwer. There was no difficulty with the fact that Frits knew nothing about science; what defeated him was the fact that he knew nothing about the international sales market. When in 1975 the five years had gone by in which, according to an agreement made with the Nijhoff family, nothing was to be changed in 'their' firm, he began with clearing the estate left by Hartgerink. In their prestigious property on the Lange Voorhout, in The Hague, with its cupboards full of unsaleable articles, they made a loss of 3 million guilders on a turnover of 13 million—at least, if we leave out *Van Dale*, the still flourishing dictionary. As if that was not a bankrupt estate... But after splitting up the business into manageable portions—the export business on which the firm had relied for years certainly required extra attention—he was at a loss. When business abroad fell and an unexpected problem emerged with the purchase of Reidel, Rienk then insisted on appointing a co-director: Chris Baltzer, who had felt out of place at Elsevier. After the departure of Bart van Tongeren in 1977, he felt that his efforts were no longer appreciated by the management, although he had been the driving force behind the successful sales of the Associated Scientific Publishers. Chris readily accepted Rienk Visser's offer to sort out matters in The Hague (Dr W. Junk was also in The Hague) and Dordrecht (the city of Reidel). Incidentally, with the purchase of Reidel it had been stipulated that after five years the management of Kluwer Academic Publishers should establish themselves in Dordrecht, a city that has nothing to offer for those accustomed to The Hague—I myself am a *Hagenaar*! Shortly afterwards Anton Reidel moved to Boston, where he owned a branch office. To return to Chris, you must understand that he was also supposed to build up the list, and for this reason promptly fetched me away from Elsevier. I was to do great things with their small sector in journals. We are talking here about 1978, when I was ready for a change. If I had stayed at Elsevier, then James Kels, the likely successor of Otto ter Haar

University, and I had been deputy director of Wolters—a school book publisher. The negotiations started with Elsevier. Elsevier's Dolf van den Brink was so sure that he could buy the shares that he was quite open about his plans to split the firm in parts, something the family could not accept. So there was no deal. Then talks started with Mijndert Ververs and Joost Kist, who represented Wolters-Noordhoff, and J.M. Gorter (not Rienk Visser), who represented Kluwer. Nijhoff's important antiquarian bookshop and agencies all over the world would have fitted very well to Wolters-Noordhoff—these companies merged in 1968—but there was no deal because of financing problems. So Gorter won. He wasn't short of capital. He had to pay a very good price, he said, and I took that as a compliment for my skill as negotiator. It was no secret that all parties were particularly interested in the acquisition of Nijhoff's well-selling Dutch dictionary *Van Dale*.'

who was already working at the New York office to prove his worth, would have got in my way.¹⁹

So much for jonker Frans. Because he was discreet, we had to learn from another source what the unexpected problem was with the purchase of Reidel: after the sale to Kluwer, Anton Reidel was unable to relinquish his position in the firm that he had built up, and hung on for a long time as director, until someone made it clear to him that he had to go.²⁰ Frans, however, had perfectly understood what was expected of him, and once he was installed at number 9 on the Lange Voorhout in The Hague, he immediately asked for the journals. Astonishment. Shouldn't he first of all set to work on the rare books, art objects and globes, precious treasures that lay neglected and gathering dust in the antiquarian bookshop? He had these accommodated in a foundation, and once more asked for the journals. Hartgerink had said, he was given to understand, that work could be done on the journals only after there was absolutely nothing left to be done on the books.²¹ After searching for some time, eventually out of a drawer appeared a piece of paper on *Plant and Soil*, a small international journal that had been published for thirty years and never been discontinued, possibly because no one had calculated its losses—the reason why *Physica* had been given away in 1951. As there was nothing to show to whom the journal belonged, he straightaway had it established as the property of Kluwer Academic.

Plant and Soil, however, belonged undeniably to a group of Dutch agronomists with a fairly large international network. It had been set up in 1947 by Eppe Mulder, director of the agricultural research station in Haren near Groningen, to discuss plant nutrition and its related microbiology of the soil.²² He had got in two editors from The Netherlands and four from abroad (from Berkeley, Copenhagen, London and Louvain), but in the beginning the journal aroused very little interest, and only after seven years, in 1954, could volume 5 be published. After the appointment of an editorial secretary, which was necessary because in 1956 Mulder was appointed professor in Wageningen, the number of contributed articles went up and filled one volume (of 400

¹⁹ Interview of Frans van Eysinga on 7 November 2003.

²⁰ See note 86 of Chapter 6.

²¹ Interview of Frans van Eysinga on 5 October 2004.

²² Historical overview in 'Henk Arnold', *Plant and Soil* **133** (1991) vii.

pages) per year. When Frans enquired about the journal in 1978, this had gone up to two per year (with a total of 1400 pages) and Henk Arnold, the editorial secretary, after twenty years of industrious correspondence with the authors, was promoted to executive editor.²³ He had done his training at the school of tropical agriculture in Deventer, and also worked for a time in tropical agriculture in Indonesia. He was a practical man rather than a scientist, and possibly this played a role in the way Frans tackled the task of rejuvenating the journal.

In fact, he decided to take on a young man who, just like Arnold, came from the school of tropical agriculture in Deventer: anyone who came from here must know how to raise plants. To this young man, Herman Spruyt, he gave the job of substantially expanding the advisory board of *Plant and Soil*, in consultation with Arnold, of course, and of publishing symposium reports in it.²⁴ Perhaps it was a stroke of luck, but the very first report to be published—on a symposium in Aleppo in 1980 on ground water and nitrogen in Mediterranean regions, edited by John Monteith and Colin Webb—immediately attracted a lot of interest. The number of volumes of *Plant and Soil* shot up to six per year, and after 1984 reached seven or eight (with a total of more than 2400 pages in an enlarged format). Thanks to this, Kluwer Academic, which upon the purchase of Reidel (in 1976, after the bankruptcy of the printing works that was part of this firm in Dordrecht) was losing money, was now able to get out of the red. Work the field and be sure that it is well nourished—once more this was the recipe to enable an inadequate crop to grow, in this case, to the size of *Tectonophysics*. Frans was thoroughly satisfied, but he was sufficiently realistic to know that journals of the size of *BBA* or *Nuclear Physics*—at least five times as large—were beyond his reach.

Spruyt brought with him yet another gift:²⁵ at school in Deventer this young man had heard of one Schilperoort, who was to carry out research in Leiden on the DNA of the causative agent of plant tumours, a field microbe. So when it became known in 1979 that this man had been made professor, Spruyt took the plunge. Rob Schilperoort remembers how this young fellow just walked into the botanical institute in Leiden, and, as if it was the most normal thing in the world, asked him

²³ Ibidem.

²⁴ Interview of Frans van Eysinga on 26 June 2006.

²⁵ Interview of Rob Schilperoort on 19 July 2006.

whether he would like to set up a journal on tissue culture for Kluwer Academic. Charmed by such directness, he answered that there were already far too many on the subject, and that there was a greater need for a journal on the molecular biology of plants. If required, he would be chief editor, and he gave Spruyt a list of names for the international advisory board, with the request that he start writing to them. All this took place in the beginning of 1980, when Schilperoort was involved in setting up an international society for the molecular biology of plants. A journal would fit in well with such a scheme. (It was still too early for a journal on techniques of molecular culture, not à la Mendel but through intervention in the genetic structure via ‘cutting’ and ‘pasting’ in the RNA—the journal *Molecular Breeding* was not set up until the 1990s, also by Schilperoort, also at Kluwer Academic.) *Plant Molecular Biology* started to come out in 1981, and that year one volume could be published. It was a busy market in which Blackwell Scientific and Springer-Verlag were particularly active—the latter quickly even tried to snatch away the journal from Kluwer, with chief editor and all.²⁶ However, thanks to its high quality *Plant Molecular Biology* grew to three volumes per year.

Books are flowers, and journals are plants.

Anyone who speaks to Frans hears him say it time and again. Flowers keep their beauty only momentarily but plants bear fruit for years, at least if they are planted in good soil and properly nurtured. Does this comparison take its roots from his origins in the Frisian nobility? Anyone who visits him at home in The Hague is reminded of these, for here hangs a unique family heirloom, a large wall map of the agrarian province of Friesland dating from 1718, surveyed and engraved by Schotanus à Sterringa.

Should the growth that the journal *Hydrobiologia* enjoyed also be attributed to his green fingers? Published at some point in 1948 by Dr W. Junk, this periodical with an international editorial board of a dozen limnologists was widely read right from the beginning. In 1978 it published five volumes per year, although it was coming out only twice a year when Kluwer purchased it from Junk. The size, in pages, grew on average by 25% per year. If this growth cannot be attributed to the increasing interest in water and water quality, then it must have been thanks to the dynamic editing of Karel Vaas who, besides being

²⁶ Ibidem.

editor, was also director of the Delta Institute in Zeeland. After his unexpected death in 1980, Henri Dumont of the Limnological Institute in Ghent took over management of the content of the journal. Growth continued, but fell slightly. Between 1978 and 1983 it averaged 15% per year, and in its last year was even negative for a while, which makes it hard to detect in it the green fingers of Frans.

And how did the other journals of Junk fare? *Documenta Ophthalmologica* and *Mycopathologia*, both set up in 1938, showed little growth, which is not surprising for these specialisms. And the other journals of Nijhoff? They were hardly noticed in the over-crowded market, both the highly respected *Genetica à la Mendel* from 1919 and *Euphytica* from 1952 that, like *Plant and Soil*, was directed at agronomists—only after a new set-up at the end of the 1980s would it grow to above the strict minimum of one volume per year.

And the journals of Reidel? Since at first glance we see few limitations that are inherent to the fields of physics and astrophysics, it surprises us that Frans didn't succeed in getting at least a few of the journals from the Reidel estate to flourish. Anton Reidel, who'd poached on the territory of Daan Frank—we refer to chapter 5—and therefore mastered 'the trick' much earlier than Rienk Visser, had launched a whole series of typically modern journals onto the market. Anyone who took a walk through the still largely dilapidated city of Dordrecht (the oldest city in Holland), paused before number 302 in the Voorstraat in front of a classic clock-gabled house, and rung the bell to ask what kind of publishing they did, wouldn't have believed it. These were journals such as *Space Science Reviews* (1962), *Solar Physics* (1967), *Astrophysics & Space Science* (1968), *Boundary-Layer Meteorology* (1970), and something else with a Latin title, *Geometria Dedicata* (1972). And then we haven't even mentioned the highly specialist *International Journal of Low Temperature Physics* from 1969 or *Hyperfine Interactions* that he took over from North-Holland in 1975, another small-timer. They were all good journals (they contained hardly any nonsense), but they didn't belong to the top. The author says this with regret, because he would so much have liked *Astrophysics & Space Science*, in which in 1979 he published his greatest discovery, to have been taken really seriously.²⁷ To find out why the growth remained limited we shall examine *Boundary-Layer Meteorology*.

²⁷ C.D. Andriesse, 'Fluctuation theory of the mass flux from the stars', *Astrophysics & Space Science* **61** (1979) 205–216.

We do not know for certain, but very likely at the end of the 1960s Anton Riedel paid a visit to Franz Schmidt, director of the Meteorological Institute in De Bilt near Utrecht, and asked him what kind of journal people in his field might need. He had already visited Cornelis de Jager, the astronomer from Utrecht, with the same question, and this had led directly and indirectly to the first three journals mentioned above. Schmidt had started out with his research group to measure wind in the lowermost 100 metres of the atmosphere (the so-called boundary-layer current), and for a possible journal would have referred him to Ted Munn of the Meteorological Institute in Toronto.²⁸ ‘The journal started out’, we read 25 years later in *Boundary-Layer Meteorology*, ‘with the small family publishing house of D. Reidel, who sent a representative to Toronto to persuade Ted [Munn] to set up a new journal.’ This was, of course, Anton himself, who from time to time needed to be at his office in Boston during that period. We see that Schmidt (the likely informant) was the only Dutchman to be included in the editorial board, and that the first article to be published is written by him. We see, too, that Munn did not commit himself straightaway, that he asked the advice of the editor of the more general *Physics of Fluids*, and consulted various meteorologist colleagues as to whether there was a genuine demand for a journal on boundary-layer currents. Then here it comes:

Whoever works in this field can find almost all that he needs to know in this one journal.... Setting up *Boundary-Layer Meteorology* with 75 volumes (and around 30,000 pages) in 25 years and keeping it going may be considered as a magnum opus [of Munn].²⁹

This is an indication of the limitation of the subject and thus the inherent size of the journal (with an average of three volumes per year). However new the subject appeared to be, and however useful it was (and is) for weather forecasting, or even climate studies, it did not rise up out of a totally new insight—through a concept of the century. And there was not a publisher in the world that could change this.

We find a second example of this inherent limitation to growth in the *International Journal of Fracture*, which Frans took over when he purchased the English language section of Sijthoff-Noordhoff in 1980 (on

²⁸ Peter Taylor, Morley Thomas, Ed Truhlar & Doug Whelpdale, ‘R.E. (Ted) Munn—Founding Editor, A Mini-Biography’, *Boundary-Layer Meteorology* **78** (1996) 3–8.

²⁹ Ibidem.

receiving permission, of course, from the directors in Deventer). This journal had been set up in 1965 by Max Williams, an engineer from the California Institute of Technology in Los Angeles, who would edit it for 31 years.³⁰ Possibly it came by accident to Noordhoff, a publisher in Groningen that—we see already—later merged with publishers Sijthoff. It so happened that one morning a Japanese professor, Takeo Yokobori from Sendai, turned up who was either too polite or too shy to ascertain whether he was indeed at the offices of North-Holland.³¹ Before he realised that he was at Noordhoff and not at North-Holland, he had already asked them if they were prepared to publish an *International Journal of Fracture Mechanics*—in name, naturally, of the group of colleagues activated by Williams. (The word *Mechanics* remained part of the title until 1973.) The director of Noordhoff, Joost Kist, had enquired of professors Warner Koiter and Egbert van Spiegel whether Williams enjoyed a good reputation. Such was the case, and the matter was agreed. It cannot be entirely ruled out, however, that Yokobori was, after all, at exactly the right address. Noordhoff had a name in the publication of mathematical works in the field of strength of materials, with Nikolai Muskhelishvili included amongst the authors. In any case, the journal was set up

...when the field of fracture mechanics was fairly small, and many doubted the necessity of a specialist journal. But Max [Williams], who had offered fruitful contributions to the mechanics of materials and structures, foresaw how a better understanding of [the occurrence of] fractures could enhance the sciences of engineering. It has become so important that it is now indispensable in practice. It is essential for the construction of aeroplanes, satellites, pressure vessels for nuclear reactors, motors, ships, and even for the design of chips.³²

The size that the *International Journal of Fracture* eventually assumed—125 volumes after 40 years—has to be seen in the light of its relationship to these applications. In fact, in its kind the journal was unique, just as unique as Williams, its founder. This enthusiastic man, 40 years old at the time of its foundation, could even explain to his publisher in the Netherlands why this profession was so useful—who, after all, would

³⁰ W.G. Knaus & R.A. Schapery, 'Dedication to Mel and Max Williams', *International Journal of Fracture* **93** (1998) ix–x.

³¹ Letter from Joost Kist to the author of 7 August 2006.

³² W.G. Knaus & R.A. Schapery, 'Dedication to Mel and Max Williams', *International Journal of Fracture* **93** (1998) ix–x.

want his missile to fall to pieces during launching?³³ With regard to content, however, this profession could not experience growth. Materials break if they are overloaded—through sudden brute force, or through a small but incessant force that has first caused a tear. It's as simple as that, and this force is either mechanical (due to motion or vibration), or thermal (due to a difference in temperature) or chemical (due to corrosion from liquid acids or salts, or gases such as hydrogen). Fracture occurs if the tension at 'imperfections' in the micro structure (and at crystallite boundaries) exceeds the binding force of the atoms, and the balance between these two can be calculated by a clever mathematical method (and nowadays, also precisely, by computer). We see that for a long time the journal only had one volume per year, in 1980 two volumes, and then gradually went up to the six volumes that we have today.

There are signs, then, of market saturation around 1980. We see them where the quantum concept was unable to make itself felt (in classical fracture mechanics and in meteorological hydrodynamics), but we do not see them, or not obviously, where the gene concept is still a driving force (in agronomics). But then—someone who has come to see the history of science as a history of concepts easily becomes short-sighted. We tried looking further, and studied an Elsevier catalogue from 1990 that contained information on the founding and size of a large number of journals with a greater or lesser connection to the gene concept. When this became highly time consuming, and might not even have come up with anything convincing, we recalled that we were writing an historical account—not an epistemological essay.

So how should the story continue? Well, Rienk Visser, who thought he knew 'the trick', saw, even in the beginning of the 1980s, little of the big profits that he had expected at Kluwer Academic. He realised this wouldn't come from *Synthese*—a small journal on the foundations of science that dated from the 1930s, which was purchased in 1959 from a publisher in Bussum and more or less re-started in 1966 with an international editorial team of 15 philosophers³⁴—but he could, or

³³ Interview of Frans van Eysinga on 26 June 2006.

³⁴ The journal *Synthese* was started in 1936, when it had the Dutch subtitle 'Maandblad voor het Geestesleven van onzen Tijd'. Its predecessor was a book-series under the same name, which appeared in the years 1914–1921, and then was taken up in *Onze Eeuw*, a monthly with subtitle 'Maandschrift voor Staatskunde, Wetenschap en

would, not understand that the tide of great science journals in the real sense was on its way out. Chris Baltzer fell victim to this; when asked to present himself in Deventer to explain why the sales barely covered the costs, he was criticised for the size of his hotel bills.³⁵ According to the Elsevier standards that he was accustomed to, these were modest, but according to those on the IJssel they were too high. Such things ought not to be said to a director of his standing, and Baltzer resigned. He went to Basel where he set up his own publishing house. We wanted to talk to him about this affront and his departure, but he was already dying by the time Frans had told us about this and we had discovered his address in Basel. After a while, in 1984, Frans became director of Kluwer Academic.

We should point out that Elsevier played a constant role in this imitation of its business, not only by setting the example and in its top people being taken over—as discussed—but also as producer of well-trained personnel for desk editing and preparing for print. ‘We were happy to help them out,’ Otto ter Haar has stated, ‘they were

Kunst’. In 1946, after the war years during which publication was impossible, *Synthese* reappeared, now in English, with volume number 5 and the subtitle ‘An International Journal Devoted to Present-day Cultural and Scientific Life’, which in 1950 was changed into ‘An international Journal for the Logical and the Psychological Study of the Foundations of Science’; the 5 volumes until 1959 were published by Kroonder in Bussum. From that year on the journal was published by Reidel in Dordrecht as ‘An International Quarterly for the Logical and Psychological Study of the Foundations of Science’, and then in 1966, after an absence of 3 years, as ‘An International Journal for Epistemology, Methodology and Philosophy of Science’, Jaakko Hintikka being the principal editor. In 1959 Hintikka had become already managing editor of Reidel’s fine *Synthese Library* supervised by Robert Cohen, Donald Davidson, Gabriel Nuchelmans and Wesley Salmon. It is to be noted that Hintikka also was member of the editorial board of the fine book series *Studies in Logic and the Foundation of Mathematics* that was published by North-Holland since 1951 and had tens of titles in the 1960s.

³⁵ Interview of Frans van Eysinga on 5 October 2004: ‘Although Chris Baltzer did the real work in making Kluwer internationally known, Frits Stenfert Kroese was the number 1. This meant that Chris had to consult Frits for every trifle and that he had to comply with the thrifty Kluwer-culture. In the eyes of ‘those provincials in Deventer’ the complete refurbishment of his office and his declaration of travel costs—both up to the Elsevier standards Chris was accustomed to—were thought to be extravagant. After 3 years, when he was confronted with much critique and little appreciation of the work he did for Kluwer, Chris decided to leave and start a publishing company of his own.’ Apparently Otto ter Haar knew more about the background of this departure: ‘The old-fashioned firm in The Hague was late in automation, very late indeed, so that Baltzer had no answer to the competition of Swets & Zeitlinger and large foreign distribution companies of journals, and on top of this, he never broke with his habit of giving substantial discounts.’ (Quote from a letter to the author of 10 January 2004.)

nice people, not serious competitors.’³⁶ We further observe that the important science journals later published by Kluwer Academic—the *Journal of Statistical Physics* and the *Journal of Mathematical Sciences*—were bought and belonged originally to Plenum Press in New York. They date from 1969 and 1972, respectively, and published articles from the Soviet literature translated by dissident Russian Jews.

This chapter cannot end without a few lines on the assault by Pierre Vinken. In 1987 Pierre—president-director of Elsevier—wished to take possession of Kluwer.³⁷ He must have been convinced that he had discovered the Cardinal Law of money just as, ten years earlier, he had discovered the Cardinal Law of information: *information generates information*. Well then, the game he saw being played all around him, especially in the Anglo-American world, may have caused another formula to occur to him: *money generates money*. In any case he had it noted down: ‘Profit with us has priority over turnover.’³⁸ But what an absurd idea, that in science publishing all should revolve around profit, that is to say, around the shareholders’ value. At any rate we hear the gnashing of teeth by scientists bound by their contracts to Elsevier who, on the contrary, would far rather see priority given to turnover, and the articles on their discoveries and insights spread across the world.^{39, 40}

³⁶ Letter from Otto ter Haar to the author of 10 January 2004.

³⁷ Lente (*Nederlandse uitgevers*) pp. 185–190. It wasn’t just Vinken’s policy. The policy was supported, of course, by his co-directors and the board of Elsevier, as they all wished more profit for international acquisitions, and it was made possible by the already leading position in the very profitable scientific journals (610 in 1985, against 367 and 257 for the numbers 2 and 3, Pergamon and Springer-Verlag).

³⁸ Interview of Pierre Vinken by Peter van Wermeskerken, Chapter 12 in: *De jaren 90, visie van Nederlandse topondernemers*, M&P, Weert (1988)

³⁹ One may also read Lente (*Nederlandse uitgevers*) p. 200: ‘He [Vinken] said that he didn’t like the European model of management, which stressed consensus and which was based, he believed, in Christian and socialist feelings of guilt and squeamishness about something very human, the desire to make profit. He declared himself in favour of the Anglo-Saxon model, which puts profit topmost on the list of a firm, *even if this firm was in the business of producing cultural goods*... Publishing was an international business, and discussions about national identity were therefore “nonsense” to him. In his inaugural lecture in 1975, when he became extraordinary professor of medicine at Leiden University, he said that Dutch was a dying dialect and that Dutch children should primarily be taught the world language English in school. This is [according to Van Lente] a very limited view. Scientific and professional publishing may have passed increasingly into the hands of people like Vinken, but literary and general publishing in the Netherlands remains an industry with many small but successful firms and many new initiatives.’

⁴⁰ Frentrop *passim*; this biography of Vinken had not yet appeared when the present text was finalized.

But more about this in the next chapter. However, he did not succeed. Even if honour is to play no role in the Cardinal Law of money, in the hearts of those who rule a family firm it does. The matter did not go unnoticed—in the eyes of the Dutch these companies were large and the bid for Kluwer high: 910 million guilders (around 500 million dollars), 40% above the share value.⁴¹ Elsevier, at the time, after its purchase and amalgamation with a number of daily newspapers, had a staff of six thousand and a yearly turnover of one and a half billion. The publishing companies that now formed Kluwer had together managed to build up a staff of four thousand and a turnover of one billion. The offer was hostile, however, following unproductive discussions on a merger. The people in Deventer had low expectations regarding the collaboration, particularly if it was to be imposed on them. To get rid of the bid—mouthwatering to the shareholders—they connived together with a smaller publisher (Wolters Samsom) who, thanks to a number of sophisticated transactions, managed to obtain a majority of the Kluwer shares. Even if Elsevier had won the battle, it would have made little difference to the science publishing of the two firms.

⁴¹ Vries pp. 183–200 [summarized by Johan de Vries, 'A Dutch Saga of Publishing Mergers and Takeovers', *Logos* 6 (1995) 124–136]; also Vermeulen & Wit pp. 62–88.

CHAPTER NINE

TOWARDS THE INTERNET REVOLUTION

In any case, hardly anything changed when, four years later, Elsevier was able to purchase Pergamon—a first-rate publishing house, the life's work of Robert Maxwell—for 440 million pounds (730 million dollars).¹ 'In fact they were in the same business,' according to Charles Ellis, who had worked in both these publishing houses, and was therefore thoroughly familiar with them.²

Pergamon was purchased long after 1980, the year we wished to end our history of these events. What comes next—the most recent past with its expansion and, not to forget, the role of the World Wide Web in the globalisation of production and electronic publishing—this is a totally different story, and a story we are not yet in a position to give an overview of; there are plenty of indications that we are currently in the midst of it. Nevertheless, in this last chapter it is worth dwelling for a moment on the acquisition of Pergamon, as well as the take-overs of Butterworths and Academic Press plus Saunders—also large, first-rate science publishers—that took place at a later date. A fivefold increase... The question is whether this strong growth just came out of the blue, or was a logical result of the history of events that we have described. In our opinion it is the latter. We think that it came about due to a saturation process in the network of competing publishers—a network that was set up at the end of the 1960s, and in the 1970s took shape in the *STM* organisation. But even if that is not the case, then we should at least outline how Elsevier succeeded in becoming the greatest science publisher in the world. Note that the name Associated Scientific Publishers was quickly dropped after the departure of Bart van Tongeren, and replaced with the name of Elsevier. Only with its many titles bearing the imprint of North-Holland would Elsevier Scientific Publishing Company, later shortened to Elsevier Science, still refer to Daan Franks's creation.

¹ This amount is given by Brian Cox in *Logos* 9 (1998) 139; Vermeulen & Witt (p. 133) give 450 million pound, as does Klaus Saur in *Logos* 17 (2006) 71.

² Interview of Charles Ellis on 4 May 2004.

What position did it occupy before these mergers and takeovers took place? For this we return to June 1968, the month in which *STM* was created, in Amsterdam, amidst turmoil and on an impulse, in the corridors of the 18th congress of the International Publishers Association. *STM*—the *International Association of Scientific, Technical & Medical Publishers*—did not originally intend to break away from the International Publishers Association, but to be part of it.³ Concern arose due to the laconic response of the congress to proposals to supplement the Berne Convention; that is to say, to undermine it. This Convention protected authors' rights, but the proposals—worded in the 'Stockholm Protocol' that was signed by states—gave developing countries the freedom to translate or copy textbooks without paying anything to the original authors and publishers. In particular, McGraw-Hill, who as everyone well knew was the largest publisher of textbooks in the world, would suffer from this freedom. Only at the last moment could the congress be persuaded to speak out against the 'Stockholm Protocol'; states could not, after all, give away the properties of corporate bodies. Thanks to this incident science publishers realised that it was not the rather slipshod International Publishers Association, but only an organisation of their own making that would truly defend their rights. So they got together. Gordon Graham, who had worked for years for McGraw-Hill and had meanwhile been asked to modernise the highly respected Butterworths,⁴ was the first to stand up and say:

I don't need to say here that there are different sorts of publishers. [He directed his words to Piet Bergmans who as host, or on behalf of Elsevier, was trying to steer this impromptu session in the right direction.] We don't publish novels. Or law books. Science publishers are in a special position. But if I examine our field, it turns out to be fairly diverse. First I see the sources of knowledge, the monographs, learned works, books for graduate students, and let me add to this the scientific journals. Secondly,

³ Letter from Paul Nijhoff Asser to the author of 10 November 2006: 'The year before (1967) the World Intellectual Property Organization ("guard" of the Berne Convention), in which the IPA (International Publishers Association) was represented, had discussed a protocol, the so-called Stockholm Protocol on a free copyright for developing countries. The IPA then didn't raise objections to the idea that developing countries had to be free to copy educational books, but a British observer had alarmed the representative of his government: *You are giving away property that doesn't belong to you! It's already bad enough that developing countries that pretend to be too poor to pay copyrights are violating these rights, and not on a small scale, but now you are going to legalise their piracy.* From then on the fat was in the fire.'

⁴ Graham pp. 68–75.

I see textbooks for schools and for interested laypeople, laboratory manuals, also popular books. Up until now these came under the educational section. This we will have to change. And thirdly I see scientific books as resource books for industry, businesses and—as long as this lasts—cultural expression. These are all very different from one another, but we could call it our three-in-one.⁵

Theoretically this is correct, Robert Maxwell said in irritation. We must define what we publish, certainly, and we must talk with these colleagues of the educational section, certainly, but I see three other things. I have come here today because a text has to be formulated for the plenary session of this afternoon. That is point one. Point two is that I'm afraid of being seen as the military man who is here just to set up a *junta*. [Laughter.] But if we do want to set up something for ourselves, then that can't happen without a self-appointed organisation committee.⁶ I propose that Piet Bergmans chairs this committee—he already maintains order here—and that it further consist of Daan Frank, also from our host country of the Netherlands, Ed Booher (of McGraw-Hill) and Brad Wiley from the United States, Georges Dunod from France and Heinz Götze (of Springer-Verlag) from Germany, and as far as the United Kingdom is concerned Robert Code Holland (of Pitman) and myself. We have each promised 500 dollars to set up a secretariat. We decided that at ten to eleven this morning. Point three is whether you can endorse this.

Upon which a disorderly discussion ensued. A Brit, or was it a Dane, called out that this was a golden opportunity to involve colleagues from the Eastern bloc, even if they couldn't care less about copyright. Everyone struggled with differences in copyright and other rights, the trade barriers.... Everyone engaged in *STM* should be able to join in! Also those who publish *Geisteswissenschaften*? Yes, said the chairman. He translated the question, since the Swiss who had asked it had spoken in German. [Uproar.] And music publishers? [In Dutch accent] I don't know yet, said Captain Maxwell, that's for the committee to decide. Ed Booher [a thin man with a moustache, fearful of the Captain's intrigues] stood up and emphatically declared that the committee would be dissolved as soon as the *STM* was set up with the blessing of the

⁵ The following three paragraphs are based on Paula Krijgsman's *Condensed Report of the Informal Meeting of Scientific Publishers, held during the 18th IPA Congress at Amsterdam, on 12 June 1968* (unclassified document of 10 pages in box 1 of the *STM*-Archive, see note 12 of Chapter 7).

⁶ The name *STM* was not used at the conference. When the secretary Nijhoff Asser started an office for the organization, in 1970, he had the name *International Group of Scientific, Technical and Medical Publishers* printed on his letter-paper and had the logo *stm* designed.

Association [which was agreed the following year in Frankfurt]. Brad Wiley, taciturn by nature, said that people did seem more or less to agree. Only since the purchase of Interscience did he count amongst the great, his family having lived for half a century in the shadow of McGraw-Hill. No, no, said Daan Frank, we don't agree at all—what will be the role of less widely-spoken languages like Czech or Dutch? The French and the Germans there kept quiet, they were lucky to be there at all. Paul Nijhoff Asser [who we already know, having met him in chapter 2] asked whether the organisation could be of use to small publishers, like himself. The upshot of this was that two years later he was able to open an office of the *STM* secretariat on the Keizersgracht in Amsterdam.⁷

All this had taken place in the Exhibition hall of the RAI, which was quite unsuited for any kind of private talk. For this reason, on the last day of the congress, Daan Frank and Bart van Tongeren invited the thirty most important *STM* colleagues, together with their wives, to a supper party at the home of Bart in Amsterdam (Daan lived outside of town). Anneke Frank and Pamela van Tongeren acted as hostess, the food was excellent, the wine superb and one of the company stood on his head.⁸ Finally all present were conveyed by calash to the closing festivities at the RAI. In this way the *crème de la crème* of international science publishing would understand that North-Holland belonged beside Elsevier, and that they both strove for an important position. Not that they were yet really great at that time. Daan, who became the first treasurer of *STM* and in 1969 drew up a list of the first 50 members, had to determine the contributions—according to financial means, while they kept their turnover a secret from him. What he chose could only be of symbolic value.⁹ He rated 1000 dollars for six publishers (Academic Press, Butterworths, McGraw-Hill, Pergamon

⁷ Until here the text is based on Paula Krijgsman's report (note 5).

⁸ Letter from Bart van Tongeren to the author of 31 October 2006: 'On the last day of the IPA-Conference Daan Frank and I wanted to present North-Holland to the science publishers at the Conference, and asked Anneke Frank and Pamela van Tongeren to invite thirty of them with their ladies at a dinner in my home. Maxwell appeared in a tropical outfit and Konrad Springer attracted attention by standing on his head to prove his new vigour after having been ill for some time. At the end of the party I had a row of coaches waiting to bring our guests to festive conclusion of the Conference, with the young daughters Deborah Wiley and Alexandra van Tongeren on the box of the first calash.'

⁹ Unclassified document of 2 pages (with Frank's handwriting) in box 2 of the *STM* archive (see note 12 of Chapter 7).

Press, Springer-Verlag, and John Wiley & Sons), 750 dollars for another six (Dunod, Masson, Saunders, Plenum Press, Georg Thieme and Van Nostrand Reinhold) and 500 dollars for yet another six (Blackwell, Carl Hanser, Elsevier, Hans Huber, North-Holland and Walter de Gruyter); of the remaining 32 members one half could be a member for 250 dollars, and the other half for 150. No one protested; they all just paid. It would not have been unreasonable. Five years later *STM* had around one hundred members, which together with their branches were spread across the whole world. The chair changed every two years, and went from Piet Bergmans to Heinz Götze, to Ed Booher, Bart van Tongeren, Per Saugman... Two Dutch names. After the merger North-Holland and Elsevier, together worth 1000 dollars, could be considered as one large publishing house, as one of the seven.

Now that the biggest players have been named, we cannot avoid examining these competitors—for competitors is what they are—more closely.

First the oldest: John Wiley & Sons¹⁰ went back to a printing works set up by Charles Wiley in 1807 in New York—Charles was a great-great-grandfather of Bradford ('Brad')—and Butterworths¹¹ went back to a London firm of printers and legal publishing belonging to Henry Butterworth from 1818. Both highly respected publishing houses. The fact that John Wiley, son of Charles, only became an independent publisher in 1848 makes this no less so. Springer-Verlag¹² is of more recent date, starting out as a bookshop in Berlin owned by Julius Springer, which he opened in 1842 and in 1858 converted into a publishing company; then Blackwell¹³ that began as a bookshop in Oxford set up by Benjamin Henry Blackwell in 1879 (but was only extended to include a publishing house in 1913 under his son Basil); then Saunders,¹⁴ set up in 1888 by Walter Burns Saunders in Philadelphia, as publishers of medical

¹⁰ Moore & Anderson pp. 2–63 (Chapters 1 & 2, on Wiley's beginnings and early years).

¹¹ Jones pp. 1–9: 'Amateur Butterworths historians have long claimed that their company dates back to reign of Edward VI, if not earlier. Credibility was lent by the fact that the firm was founded in 1818 at No. 7 Fleet Street, believed to have been the same building "within Temple Bar" at which one Richard Tottel opened his [legal printing] business in 1553.'

¹² Sarkowski pp. 1–47.

¹³ Norrington pp. 1–70 (Chapters 1–7, on the Blackwell-history from 1879 to 1924).

¹⁴ Dusseau pp. 17–26: 'Why did Walter Saunders go into medical publishing? As a business venture, of course, but behind his decision was sound knowledge acquired

literature; and finally the publishers of technical literature belonging to James McGraw and John Hill, both set up in 1891 in New York, which, after the death of the latter in 1916, became McGraw-Hill.¹⁵ Of these five houses dating from the second half of the nineteenth century, three quickly showed strong growth: that of the Springer family immediately after the French-German war and those of McGraw and Hill thanks to the period of expansion after the American civil war. Engineers were in the leading roles.¹⁶ Fritz Springer, son of Julius, was an engineer—no wonder that he published technical journals, in addition to the pharmaceutical and chemical list built up by his older brother Ferdinand (Sr). Similarly, John Hill, the publisher of books and journals on locomotives, railways, mine installations, electrical machines and in fact everything discovered by Nikola Tesla and Thomas Edison, was an engineer as well. And again, so was James McGraw, a tight-lipped, forbidding character. William Wiley, son of John (one of the sons) who, after his terrible experience as a soldier in the civil war, published *Engineering, Materials, Bridges etc., Hydraulics etc., Steam Engines etc.*—he too was an engineer, but with his staff of ten no match for James McGraw, who in 1916 was able to employ 200 staff at McGraw-Hill to print 150,000 technical manuals for the American army and to dispatch these in crates of a hundred to France for the army fighting there.¹⁷ But let us be mindful not only of engineers, nor even only of *STM*. The highly respected Butterworths on London's Fleet Street, which, following the death of Joshua, son of Henry, was sold to two brothers, Charles and Richard Bond, published only legal works for a century—with such success that it never even entered the head of a descendant, Stanley Bond, son of Charles, to publish anything else. On the contrary, it should be more of the same and in the 1920s and 1930s he went on to publish such works as *Halsbury's Laws of England* and other books of reference to

from the used books he handled. [...] He recognized the urgency of [the application of] new inventions in preventive and curative medicine and did something about it.'

¹⁵ Burlingame pp. 469–474 (Overview publishing activities by Hill and McGraw 1891–1917).

¹⁶ Sarkowski p. 108 (papers by Werner Siemens, the engine of Rudolf Diesel); Burlingame p. 470 (the *American Machinist* and the *American Electrician*); Moore p. 87 (on William Wiley 'the Major').

¹⁷ Moore p. 103: 'The dramatic expansion [of McGraw-Hill] was accelerated by engineering handbooks patterned after those pioneered by Wiley—twenty books designed specifically for the armed services during World War I, and a special order of 150,000 books (all neatly crated in 1,500 cases in less than ten days) to be distributed to doughboys in France by the American Educational Commission.'

be exported en masse throughout the British Empire, thereby earning its name of jewel of the Commonwealth.¹⁸ Only after the firm was put onto the stock exchange in 1946 and Hugh Quennell was put in charge did it venture into science and technology publishing.¹⁹ With Blackwell the story was similar: Basil Blackwell only ventured upon the path of science in 1939, when he founded Blackwell Scientific²⁰ which, in spite of its grand name, was only intended for doctors and medical specialists and only really started to flourish after 1952 with the appointment of a Dane, Per Saugman.²¹ Therefore, two of the nineteenth century publishing houses that we have named only entered the field of the *STM* half way through the twentieth century. And, in

¹⁸ Jones pp. 78–95.

¹⁹ Jones pp. 130–150: ‘The scientific advisory board planned an advance on two fronts, both of which implemented the *Governments purpose* to develop British scientific publishing through the medium of *Butterworths*.’

²⁰ Norrington p. 135 and pp. 164–167; Saugman pp. 9–22.

²¹ Interview of Per Saugman on 2 March 2004. How would it be to talk to a man who in many respects could be compared with Daan Frank? Both started as apprentice in a bookshop and then succeeded in building an important science-publishing house out of almost nothing: small companies employing half a dozen people. The main difference seemed to be, that Per (1925–2006) in the end had enough energy to write a history of ‘his’ house [*From the First Fifty Years*—see the Bibliography] as well as his memoirs [*The Way it Was*, Sunningwood House, Oxford (1994)], whereas Daan (1913–1995) only left incomplete notes on his North-Holland and on his life. Both had fathers that were physicians, and both were charming personalities. Daan became apprentice in 1934, Per in 1941. Daan started his career in North-Holland in 1937, but the war intervened so that the real start was only in 1945. Per started his career in Blackwell Scientific in 1949, after he had left Denmark (where he was born and was to die as well), but the real start was in 1951, when he got the job of sales manager. Daan’s breakthrough came in 1956 with Léon Rosenfeld’s enormously successful journal *Nuclear Physics*, and Per’s came almost at the same time, in 1955, with Sheila Sherlock’s textbook *Diseases of the Liver and Biliary System*, with its 8 editions and tremendous sales (almost half a million). The book by Sherlock established Blackwell as the number 1 medical publisher in the U.K., after Basil Blackwell’s start on number 22. Speaking about the cause of his success, he said: ‘One has to be careful and honest, that is what scientists like, and to work hard. In the beginning I was travelling most of my time to speak to potential authors, listening to their ideas and adding those I had myself. For example, I approached Arthur Rook with the idea for a special book on paediatric dermatology, but he said to have a better idea, namely a multi-volume textbook on dermatology in general, and invited me to a dinner where I was unexpectedly confronted with 36 dermatologists he had invited as well and were ready to contribute to it. There and then I had to improvise how I could handle such a book, not even convinced that it would sell well—as happened to be the case, with until now 4 editions—and even bluntly refused colour illustrations because they were expensive. By this quick decision I robbed Rook from Edward Arnold, my competitor, his regular publisher. In a similar way I robbed excellent Scottish authors from Livingstone in Edinburgh, who thought that Scottish authors belonged to him.’

fact, the same also applied to Springer-Verlag. It took until 1964, just before the death of the all-determining Ferdinand Springer Jr, before Heinz Götze succeeded in opening an office in New York and abandoning German—before that time Springer-Verlag, in spite of its great qualities, made no headway in the new *lingua franca*.²² And Saunders? Yes, Saunders did. After the death of Walter Burns and an interim management on behalf of the widow, son Lawrence managed large print runs of Russell Cecil's *Textbook of Medicine* and the monumental *Operative Surgery* by Warren Bickham. In 1948 he even had a bestseller: the Kinsey report *Sexual Behaviour in the Human Male*.²³

But things went rather strangely for Saunders. When Lawrence died the firm came into the hands of the controversial Tiffany network—the common name of CBS (Columbia Broadcasting System) that wished to invest its profits from its broadcasting programmes in successful publishing companies and therefore also bought up *Woman's Daily*.²⁴ This was all in 1968, the year in which *STM* was created, but no one from Saunders was present there in Amsterdam, because of the uncertainty about its future. This would only become secure four years later, with the appointment of Harry Most as director, but Tiffany would always be looking over his shoulder and keeping an eye on the profits (as they did with Holt, Rinehart & Winston and other firms they'd bought up). And then fourteen years later the Tiffany network wanted to get rid of these unfamiliar activities. Could this be just the first example of the road that science publishing must take? The road on which each one of them would come up against some power that might trade in their contribution to science for some prevailing fashion? It seems so. For let us take a look first at Academic Press, which only became established in New York in 1942, but was actually already an old company.²⁵ It was

²² Götze pp. 74–165 (Chapter on Overseas Branches): 'Such old friends of the firm as Richard Courant were convinced that expansion to North America was unavoidable if the company wanted to regain its world renown.' (p. 87)

²³ Dusseau pp. 59–82, 42 & 103, 123–140.

²⁴ Dusseau p. 231.

²⁵ Morris pp. 61–63 (Chapter on History of Academic Press). We quote the following lines: 'The AP publishing philosophy, as set down in the Jacoby era, is one of "streams" or "branches" from a central source. This strategy identifies an important core subject—usually a specific area of life sciences, such as enzymology—and then covers it in an intensive way. This coverage involves not only individual books but also serials, journals, and monographs as well. A key element in this strategy is the issue of timeliness [...] A good science publisher therefore must not only evaluate the current research in a given field but also anticipate where research is leading and then react

a continuation of the Leipzig firm of Kurt Jacoby and Walter Johnson (mentioned in chapters 3, 4 and 5), Akademische Verlagsgesellschaft, which with their high standard of publications, quickly became a household word. A household word at least in biological science, in which Kurt specialised and for which he set up important journals, such as the voluminous *Virology* (edited by Salvador Luria) and the *Journal of Molecular Biology* (mentioned in chapter 6). When Kurt died in 1968 at 75—so, once again in our *STM* year—his brother-in-law Walter sold practically all the shares and they fell into the hands of Harcourt & Brace, itself no small company. Alfred Harcourt and Donald Brace had laid the basis, there in New York, for the publication of cheap books (paperbacks) for a wide public, and then the man whom they had chosen as their successor, William Jovanovich, made it thirty times as big in as many years with his rather controversial purchases. In fact, it was no longer a publishing company, but a conglomeration of insurance companies and a theme park, *Sea World* in San Diego. And sure enough, after a while Academic Press was gradually transferred to San Diego. Fortunately in the last years of his life Kurt Jacoby had trained people to take over his tasks, James Barsky for the new acquisitions and Roselle Coviello to take over the hundred and something journals, the fantastic scientific capital that he had left behind...²⁶

This story continues, and will merge with that of Saunders, but before it can be told we need to return to the end of the 1960s. In fact, the future of Butterworths also turned out strangely, for although Hugh Quennell tried to put Butterworths on a more solid basis with scientific, technical and also medical publications, these could not compete against specialist publishers in these fields. Nor did they fit into the old-fashioned style and outdated techniques used by the house for its legal publications. So while Butterworths was in this weak position Robert Maxwell made an offer. Horrified at the thought of Maxwell, Quennell's successor couldn't rest until he had placed the jewel of the Commonwealth into the hands of ... a newspaper concern, a newspaper concern in London, that is, where the prevalent craze of the day held sway. This was in 1967. And nor did this change when Reed took over the concern, three years later, for Reed was a large publisher of

accordingly. This is a likely explanation for Jacoby's spending so much of his day talking to current or potential authors.'

²⁶ Idem.

business magazines. We have no need to describe here how this firm had started out as a modest *Newsprint Mill*, set up by Albert Reed in the countryside just outside London, for (although it will come up again further on) science plays no part in it. But this prevalent craze of the day... had it somehow also taken root in Maxwell's brain when his attempt to purchase Butterworths didn't come off? It seems it must have; otherwise one doesn't gamble away one's company. We cannot, therefore, pass over the almost unbelievable story of what happened to Pergamon Press. In 1951, after he had bought out Pergamon from the two firms with whom he started out, Butterworths and Springer, and, in 1956, got rid of the partner to whom the firm owed its good name²⁷ (the much older, refined Paul Rosbaud, who couldn't bear to see how the Captain, entirely on his own bat, chased after authors at the Geneva Conference on nuclear energy), Captain Maxwell ferreted out the secret on how to grow (getting in before anyone else and spiking their guns by setting up vast numbers of journals, hit-and-miss, like *Tetrahedron* and a whole series of other specialised journals, each beginning with *International Journal of ...*, forty at least, and then with the flood of cash this unleashed immediately raking in famous names in science). He subsequently moved his whole company into Headington Hill Hall in Oxford where, seated in the largest room, he had his visitors traverse an acre of knee deep carpet before they reached his desk.²⁸ That was in 1959. His megalomania manifested itself even more obviously in *The Commonwealth and International Library of Science, Technology, Engineering and Liberal Studies* that contained more than a thousand titles and all the best authors, and also in the proceeds he reaped from his great stack of shares when he put Pergamon onto the stock market as a public company, and perhaps most of all in his candidacy for a seat in parliament (which he managed to win, and keep for six years until 1970). We have already seen how he was behind the establishment of the *STM* in Amsterdam. This was a climax in his career. Half a year later marked the beginning of his disastrous relationship with Saul Steinberg. This man leased computers—quite new in the 1960s and still costly—and to this end had set up Leasco Data Processing in New

²⁷ The remarkable name of the company, Pergamon, was conceived by Heinz Götze, a man of great erudition who had studied art history and was familiar with antiquity (Götze p. 17).

²⁸ Brian Cox, 'The Pergamon phenomenon 1951–1991: A memoir of the Maxwell years', *Logos* **9** (1998) 135–140.

York. Robert Maxwell and Saul Steinberg bought up shares in one another's companies. If they could succeed in coupling the information at Pergamon to the computers of Leasco, then they would have a unique position in the market, and each kept up the illusion towards the other that this was perfectly feasible. But they quarrelled. Even in 1969 Steinberg realised that Pergamon was a company full of hidden flaws, so he lodged a complaint with the London stock exchange, who decided that the restless and unpredictable Maxwell was *unfit* to lead a public company. The shocked Captain had no other choice but to resign.²⁹ Then the banker of the company, Rothschild, phoned Bart van Tongeren and asked his advice—in fact, he asked him whether Elsevier would like to take over the company. Bart said: in principle, yes, on condition that all the booby traps be removed beforehand.³⁰ The bank was unable to guarantee such a thing and the first opportunity to take over Pergamon passed through the fingers of Elsevier, leaving the publishing house in the hands of what was, in fact, a computer leasing firm.

Naturally there was much talk in the publishing world about these four great takeovers, all in a row. After some time it seemed that it would happen all over again, with *STM* devoting a whole meeting to the question in Frankfurt, in 1987.³¹ The banker who was called in for the occasion, Kendrick Noble from Payne Webber in New York, contended that such takeovers were part of a wave, whipped up by an availability of large amounts of cash and a cyclically determined thirst for information: a kind of natural phenomenon, with fluctuating variables such as interest and a somewhat dubious takeover value as parameters. We have been unable to discover just what the publishers gathered there in Frankfurt thought of this explanation, but for us it holds little appeal. We saw no economic wave phenomenon, but only caesura in the leadership of publishers, caesura that by chance occurred

²⁹ Elisabeth Maxwell (*A Mind of my Own*) pp. 382–385.

³⁰ Letter from Bart van Tongeren to the author of 23 October 2006: 'When Maxwell sold the shares of his company in England, he had silently transferred important rights to his American establishment (in particular those to sell back-volumes of the Pergamon journals), while that establishment was carefully kept outside the deal with Saul Steinberg.'

³¹ J. Kendrick Noble Jr., *The Book Publishing Merger Wave: Why it began, when it will end, and its implications*, International Group of Scientific, Technical and Medical Publishers, Amsterdam (1987).

at roughly the same time, and which at the end of the 1960s also had nothing to do with the saturation effects of the 1980s.

This brings us to the activities of *STM*. In spite of all the conflicts between the members of this organisation—they were, after all, competitors—there was also a need to discuss common problems. Previous meetings had been devoted to budget-monitoring, cost savings, licenses, reprography, possibilities within the field of electronics (such as read-only-memory, for which they wished to take a common viewpoint), the Berne Convention (which, newly worded in 1970, was aimed at protecting scientific, literary and cultural works), and suchlike themes.³² This periodic exchange of information and points of view was constantly nourished and sustained by Paul Nijhoff Asser, the *STM* secretary, whose task it was to make the *STM* market ‘honest’, as members naturally wanted to be able to compete in each other’s countries under the same conditions. So it was Nijhoff Asser’s task to bring to light the differences in these conditions—differences in the protection of intellectual ownership, in particular authors’ rights, taxation (import rights and fiscal measures), and measures against censorship and pirating that went with it—and then to draw up memoranda to ensure the safety, internationally, of the rights of science publishers. To this end he acquired some fame for his ironic article about an American regulation: ‘The Gutenberg Bible was published in the USA: Legal deposit in the modern world’.³³ These memoranda were added to an extensive diplomatic correspondence with the members, in which, amongst other achievements, Gordon & Breach were not admitted, due to impropriety and possibly also piracy.

It was because Maxwell belonged to the founders of *STM* that the participation of Pergamon could no longer be put to the vote, but his pirating practices were legendary and would otherwise undoubtedly

³² The pertinent *STM*-brochures include: Engelbart van Tongeren, *Excerpts from the Report presented to the 20th Congress of the International Publishers Association* (1976); Paul Katzenberger, *Copyright and Reprography* (1978); Per Saugman, *Regional Aspects of International Publishing* (1979); John Hanley, *The Changing Reader* (1983); Lynette Owen, *To License, or not to License* (1986); Irving Horowitz, *Expropriating Ideas: The Politics of Global Publishing* (1986); Jon Baumgarten & Christopher Meyer, *Effects of the U.S. Adherence to the Berne Convention* (1989).

³³ *Learned Publishing* 3 (1990) 1–9 [Journal of the Association of learned and professional Society Publishers]; see also Paul Nijhoff Asser, ‘Consolidation, Internationalization and the Future of Publishing: A Scenario’, *Book Research Quarterly* 5 (1989) 51–59.

have been an obstacle to his becoming a member.³⁴ Who was unaware of how he had poached authors from others at the Geneva Conference on nuclear energy in 1955? It is described in chapter 5, and really nothing needs to be added. But to show that this kind of behaviour never really changed we give a later example, which comes from Klaus Saur, publisher from Munich and *STM* member, who we visited to talk about science publishing in Germany:

'I used to publish *World Patent Information* on behalf of the European Commission,' said Saur, 'and one day I had to announce to the Commission that I had to raise the subscription by 25%, or else reduce the number of pages by 35%. Neither was acceptable, I was told, and they had already been in touch with another publisher who could satisfy their conditions. Then Maxwell telephoned. He offered me a reasonable price for it, 25 thousand marks, and asked for my Swiss bank account. "I don't have a Swiss bank account," I answered. But he pretended not to understand and asked me again for my Swiss bank account. He wanted to manoeuvre me into a position in which he could blackmail me, you understand? After the money was transferred into my bank account in Munich, *World Patent Information* appeared once again, twice as expensive, with half the number of pages and hideously printed. Then I wrote to the Commission to say that I, too, could have doubled the price, and halved the number of pages, but I could never have printed it so badly.'³⁵

It paid to be cautious when doing business with him; that is the message of this story—and of countless others.³⁶ But businessmen, who know how to be cautious, had also to fear an evil spirit... There was definitely something demonic about Maxwell where *Der Wille zur Macht*

³⁴ Letter from Paul Nijhoff Asser to the author of 10 November 2006: 'I met Maxwell around 1978, by chance, since he never came to *STM*-meetings. I always did business with Gilbert Richards, one of the good people he had in Oxford. The man had grown incredibly fat and inspected me from under his formidable eyebrows, then asked: *Do you know who I am, and that I founded STM?* Yessir, how could I forget? *Couldn't you arrange a position in the board for me?* I'm sorry I can't, Mr Maxwell, but when you come to our meetings, people will get accustomed to you again and maybe they will ask you one day to become member of the board. *Pffff*... he said, turned around and legged away. But he didn't terminate his membership.'

³⁵ Interview of Klaus Saur on 4 February 2004; his company, founded by father Karl in 1949, specialized in the publication of archives, bibliographies and handbooks [see Titus Arnu, Andreas Brandmair & Konrad Kratzsch (Eds.), *Chronik und Bibliographie 1949–1999*, K.G. Saur Verlag, München (1999)]; Klaus' view on the current situation of German professional publishing is resumed in: 'Stable, disciplined and still in the family', *Logos* 17 (2006) 68–75.

³⁶ The other side of the transaction is described by V.S. Dodd in: Elisabeth Maxwell (*Robert Maxwell & Pergamon Press*) pp. 623–625.

was concerned. After being forced to stand by and watch for five years how Saul Steinberg brought Pergamon, his own creation, to wrack and ruin—the shares decreased steadily in value, and the authors began to look for other publishers—he saw an opportunity to buy back the company, with the support of a few investors.³⁷ The following year, 1975, at the beginning of its recovery, he had appointed an American, first as director of sales, and a short while later even as co-director. But his demonic tendencies had driven this newcomer away, lured him back, then driven him away once again, which led inadvertently to the appointment of a go-between for Elsevier and Pergamon. This is why, in our history of Elsevier, the story must also be told here about this even-tempered and amiable Charles Ellis. We looked him up in Paris, where he had gone to live with his French wife after a career that had brought him from Science Research Associates in New York to Pergamon in Oxford, then to Elsevier in Amsterdam, back to Pergamon, then to Elsevier in New York, and finally in the same city to Wiley. This is what he had to say:

Robert Maxwell had so many other interests besides his publishing business that after a few months he put Gilbert Richards and myself jointly in charge of Pergamon. However, Maxwell turned out to be unable to delegate, so that working together degenerated in conflict. I couldn't get on with him. So I looked for a way out. This came from Otto ter Haar who offered me the position of sales director of Elsevier Science in 1977. I understood this to be Chris Baltzer's job, who had since left. As I thought that I would be staying in the Netherlands for quite some time, I went and learnt the language with the nuns at Vught, but by 1980 I had already left again. You see, Robert had let me know that he deeply regretted our quarrel. I was needed in Oxford, he said. He would hand over to me his position as chairman. He seemed serious. After his wife Betty had also brought her influence to bear on my wife Sandra—the two were good friends—we left Amsterdam. But the moment we got to Oxford, Robert changed his mind again. There was no question of his

³⁷ Haines pp. 297–334 (Chapter 8); Maxwell's bid for Pergamon shares in January 1974 was equivalent to 1.5 million pound; more than 90 percent of shareholders accepted the offer, including Steinberg, who had been ready to pay 25 million pound for Pergamon in 1969. In a letter of 23 October 2006 to the author, Bart van Tongeren gave the following comment: 'Steinberg had little to do with the downfall of the company. There were hidden problems. One of those boobytraps was a company on Bermuda, owned by Maxwell, not by Pergamon, to which journals were delivered at a high discount and subsequently sold to Pergamon's establishment in America with a low discount, while Maxwell put the difference in his pocket. There were also persistent rumours about secret bank accounts in Vaduz (Liechtenstein).'

resigning his chairmanship. Once again, it was Otto who offered me a way out. Now I could be director of the Elsevier branch in New York, which had its own list of technological and (clinical) medical publications, some with advertising. I led this branch for eight years, as far as possible according to the profit philosophy of the chairman in Amsterdam, until 1988 when Bradford Wiley invited me to become chairman of his family publishing firm. A historical company from 1807, you know that, don't you? John Wiley & Sons was smaller than Elsevier, but with its staff of 3000 it was quite something. A significant part of the staff was highly educated. Besides the department of science journals, which after a recent takeover (by Liss) was extra strong in biochemistry, it had departments for university textbooks and for a general reading public. I was to see Wiley through the battlefields of electronic access and threatened takeovers.³⁸

This rhetoric about battlefields shows just how seriously people took the challenge of the new media, namely the World Wide Web that was just around the corner, and which scientists could use to exchange results without a publisher being involved. At the *STM* meeting in 1984 they had already considered ways in which publishers could exercise some control over internet access 'in which they have a legitimate interest.'³⁹ Arie Manten had spoken on this occasion on behalf of Elsevier. Gerry Brown, deeply involved at the time with editing *Nuclear Physics* and therefore also with developments in CERN, in which people were working hard to establish the internet, found that Elsevier was on the late side in its response to the new opportunities.⁴⁰ Again according to Brown, the Executive Board had failed to grasp the significance of electronic publishing altogether, and therefore the deadly danger that it posed—the danger, namely, that scientists would be able to manage

³⁸ Interview of Charles Ellis on 4 May 2004; Ellis could compare the three companies he had worked for: 'They are in the same business. The success of these companies is independent of their size, since all three are large enough to guarantee very strong positions in distinct fields of research that are in strong development. Although Elsevier became four times larger than Wiley after its acquisition of Pergamon, it is no real threat to Wiley's independence, if only because a further growth might be stopped by anti-trust laws that are effective in Europe and America. Scientists who call Elsevier and Wiley monopolists with scandalous profits have no idea of the costs of proper refereeing, improving, marketing, and distributing what they write. Let them try to publish their work on their own. Then they will find out.'

³⁹ 'Original publishers will have a legitimate interest in the control and protection of works that they have published when they are disseminated through other [i.e. electronic] channels.' Declaration by Frank Cavanagh, John Davies, Willem Koops, Alexis Koutchoumov, Klaus Lehmann, Maurice Line, Arie Manten, Paul Nijhoff Asser, Manfred Seidel & Lars Tynell, *A Joint Statement of European Librarians and Publishers, STM* Amsterdam (1984).

⁴⁰ Interview of Gerry Brown on 17 March 2003.

without the journals from which the science publishers lived in such princely fashion. If this is true, then it is also true that at the end of the 1980s the Board did see that millions should be invested in the digitalisation of all the documentation—and agreed that this should now be done quickly—in a manner that fitted in as well as possible with the working practices of scientists, who as a whole were not nearly as far advanced in the use of the internet as the nuclear physicists.⁴¹ Due to a variety of practical reasons the transfer to electronic publishing required many years, however energetically the chairman, with all his experience in computerization and information systems, pushed it. Pierre Vinken saw substantial profit in it and he made no secret of wanting to increase profits at Elsevier each year by 20%, ‘which has succeeded pretty well for more than twenty years.’⁴² This 20% was not sacred, but should be seen as a challenge. He said this in 1987 in an interview. Because this was so high (under the changed circumstances in which symptoms of market saturation were already visible, we add, while incomes were also falling in parts of the holding that had nothing to do with science publishing), the firm should not only stay ‘athletic, agile, efficient and alert’, and carry on bringing new products onto the market, but buy up other firms as well: ‘The acquisitions form a closing entry [to reach the 20%], but nevertheless an important entry.’ It is such a popular argument, and also so simple that any chairman could have thought of it, and most probably had long done so. The takeovers of the 1980s, in which Elsevier had wanted to partake when it made a bid for Kluwer, can therefore be fittingly attributed to the inertia of keeping to the old, trusted profit streams *as soon as the development of new products cannot provide them*.

This clarity of purpose in Elsevier stands in contrast to the more cryptic aim to be ‘athletic, agile, efficient and alert’ in its performance. Both the new electronic resources as well as the changing market must have meant great change, but what exactly? And what was ‘alert’? Because

⁴¹ Letter of Otto ter Haar to the author of 24 October 2006: ‘Our eager beavers were too early. We [the Elsevier management] had to tell them to be patient until the company as a whole, with all its departments and establishments abroad, was ready to handle the new way of information transfer [by the web] which, among other things, had far-reaching financial and commercial implications. Thanks to the timely investments, we were the first of the large science publishers to provide access to our journals on the web. Pierre Vinken’s role in this innovation should be acknowledged.’

⁴² Interview of P.J. (Pierre) Vinken by P.C. (Peter) van Wermeskerken, published as Chapter 12 in *De jaren 90—visie van Nederlandse topondernemers*, M&P, Weert (1988).

this history was to go only as far as 1980, we haven't collected the information to answer this. Otto ter Haar, who was director during this time of change, recalled first and foremost the human dramas entailed in the abolition of hand work, such as desk editing and typesetting. 'It still saddens me when I think of how we had to let Northprint in Meppel bleed to death, the factory that for dozens of years had kept going for us at full power and each time managed to employ better IBM machines. But in the 1990s the whole world went online, so we had to.'⁴³ Then he mentioned the replacing of magnetic tapes, the arrival of electronic storage and read-only-memories, personal computers, the camera-ready copy that was demanded of authors, automation, scanning techniques, the coupling of blocks of electronic information with classically printed prestigious journals, and with all this the loss of jobs and the new skills that had to be developed, so the organisation had to continuously adapt. And the former director of Elsevier Science wanted to get this off his chest as well: 'How often did I have to hear it said that scientific journals would become an anachronism? But I don't see them ever disappearing, because although unimaginable amounts of information can be conveyed by internet, it can never take the place of the archival and bibliographical services these journals provide.'

Here we shall put aside the history of Elsevier for a moment, and follow that of Saunders and Academic Press, which in 1968 came under the ownership of the Tiffany network and Harcourt Brace Jovanovich, respectively. Of all the works Saunders published during the years it was part of the network, we mention only the brilliant book on heart disease by Eugene Braunwald, and the superb *Atlas of Medical Anatomy* by Martinus Woerdeman—an author who we met in chapter 6—but now edited by Jan Langman (it went back to the 1930s, when it was not yet in colour). As we have said, in 1986 the network wished to get rid of Saunders and sold this publishing firm, together with Holt, Rinehart & Wilson, to Harcourt Brace Jovanovich. However, these takeovers proved so expensive that the purchaser got into financial difficulties. The large conglomerate that originated in San Diego, and which now included not only Academic Press but also Saunders, began to falter,⁴⁴ and when this became known in 1990 Robert Maxwell made a bid for it. But during the course of the 1970s and 1980s this man's reputation

⁴³ Letter from Otto ter Haar to the author of 24 October 2006.

⁴⁴ Pruitt pp. 201–242 (Chapter 7).

had not got any better. The way in which he had managed to get hold of the newspaper concern of the *Daily Mirror* was just as dubious as the financial basis of the Maxwell Communication Corporation in which he had accommodated Pergamon, the *Daily Mirror* and his countless other companies.⁴⁵ ‘Everyone’ knew as much. To avoid abandoning Harcourt Brace Jovanovich to such a bleak future, the chairman (no longer William Jovanovich) endeavoured to secure a merger with a transparent, genuinely capital-rich company. This was to be General Cinema Corporation that, after considerable hesitation by the shareholders, decided on a bid of 1.56 billion dollars, making it Harcourt General that shortly after employed Pieter Bolman, who we know from chapter 6, and put him in charge of Academic Press. Maxwell’s bid, which had been 10% higher than the 1.56 billion dollars, fell through. After paying 2.7 billion dollars for MacMillan in New York, the man got into serious difficulties. Otto ter Haar writes:

Maxwell’s irrepressible ego had landed him in a really tight corner. His takeovers in the United States had been much too costly, and what proved to be his undoing was that he lost the shares that had served partly as security for the enormous loans to finance these takeovers. In a last attempt to pay off his debts he offered Pergamon for sale. That was in 1991. Pergamon was the apple of his eye, the basis of his business success, and the fact that he had to give it up clearly shows the straits he had got into. We knew the company well. It was slightly smaller than Elsevier Science, but it published the same kinds of books and journals, so that we could work out pretty well how much profit they made there. Maxwell had little time to sort out his finances, we knew that, and we assumed that the others—Wiley, Springer and Harcourt General—wouldn’t stand in our way. They knew it less well and were undoubtedly afraid of hidden flaws. McGraw-Hill stayed out of it altogether as they only published books.⁴⁶ The price we agreed on, 440 million pounds (1200 million guilders) wasn’t bad at all, under the circumstances. In 1987 we had made a bid of 910 million guilders for Kluwer, which was much smaller and

⁴⁵ Elisabeth Maxwell (*A Mind of My Own*) pp. 473–474: ‘Yet the price of his victory—some \$3 billion in loans at the start of an international recession and economic crisis—brought with it suffocating financial pressures, which got only worse in the years to come.’

⁴⁶ Quotes of Curtis Benjamin, who from 1946 to 1966 was president of the McGraw-Hill Book Company: ‘The rate of new-book production has fallen short of both the rise in book sales and the overall growth of our national economy—the prospect of continued growth in book-reading aged populations is very bright.’ (Benjamin pp. 66, 73) ‘Without an abundance of books—more and more of every kind for more and more readers—the world would surely become a stale and dreary place indeed.’ (Benjamin p. 157)

actually didn't suit our plans at all. If we combined the management, acquisition, administration, housing and mailing, then this would mean large savings. However, now that we had become almost twice as big in one go, and therefore clearly the largest *STM* publisher in the world, we were accused of monopoly formation. We were even afraid that the sale would be prohibited, but the libraries in the United States that wanted the government to forbid it were too late with their procedure so didn't succeed.⁴⁷

Incidentally, the 440 million pounds that Robert Maxwell got for his Pergamon was not enough to pay off his debts. He even plundered the staff pension fund, and when that still wasn't enough he went under, destroyed by his own demons.⁴⁸ That was in the same year, 1991. His widow Elisabeth maintains that he met his end under unexplained circumstances in the waves of the Atlantic Ocean.⁴⁹

And what happened after the takeover? If the directors of Elsevier Science found this to be the wrong moment for a generous increase in the price of journals, then it followed on very soon afterwards. For why wait when Wiley, Springer, Academic Press and the other *STM* publishers had all raised prices in the early 1990s. They were smaller, but could just as easily be accused of monopolistic behaviour. There seemed to be a law that induced them one by one to substantially increase their journal prices. But who was able to fathom this law? We, in any case, were unable to find any comprehensible explanation for the causes and results of the 'serial crisis' to which it led. Let us first quote a Californian librarian, who in 2002 addressed himself to no publisher in particular:

The serial crisis is real, but much misunderstood. There is no crisis in the publishing and acquisition of journals in the arts and humanities. Those journals are reasonably priced and are not subject to steep increases in price. Scientific, technical and medical journals are quite another matter. Many academic libraries have responded to the high subscription prices and the 10 percent plus annual increases of *STM* serials by cannibalizing their book and other material budgets, the serial budgets in other areas, and by across the board cuts that unfairly penalize the arts, humanities, social sciences, and professional disciplines.⁵⁰

⁴⁷ Letter from Otto ter Haar to the author of 10 August 2006.

⁴⁸ Bower *passim*.

⁴⁹ Elisabeth Maxwell (*A Mind of My Own*) p. 3; but read also pp. 512–516.

⁵⁰ Abel & Newlin p. 260 (in the Chapter 'The Economic Crisis in Libraries' by Michael Gorman, Dean of Library Services, California State University at Fresno).

So what is the position with regard to the costly *STM* serials? A price increase of 10% per year will, in ten years time, make them more than twice as expensive. For if we calculate $(1.10)^{10}$ we find a factor 2.6. The figures we have at our disposal are those of Elsevier journals, but are, as we have said, not specific to this company. In 1970 a volume of *Nuclear Physics* cost 60 guilders, in 1990 it cost 400 guilders, and in 1998 it would cost 1004 guilders. In 1970 a volume of *Biochimica et Biophysica Acta* cost 69 guilders, in 1990 it cost 233 guilders, and in 1998 it would cost 540 guilders.⁵¹ These volumes are comparable, being of similar size and all coming out about once a week. This means that for *Nuclear Physics* the price in the period 1970–1990 increased by an average of 10% each year, but in the period 1990–1998 this went up to 12%. The annual percentage increases for *Biochimica et Biophysica Acta* were respectively 6.3% and 11%. Does this mean that after 1990 they overstepped the mark? In the eyes of the librarian of our Faculty they certainly did. When in the course of the 1990s his budget became too small to pay for all the journals we used to read, he chose to cancel the subscriptions of the most expensive ones, which happened to be those of Elsevier—to be sure, only after lengthy consultations with staff members about price-quality ratios.⁵² Such choices were made at other laboratories as well, although not everywhere. The question of price increases had another side, of course. There must have been economic necessities for the publishers as well. Were too many subscriptions cancelled, forcing up the price for subscribers, because in the meantime the Internet offered an alternative? Any researcher could send his discoveries into the world and exchange ideas directly with colleagues via the Internet without the intervention of any journal's editorial office. If this is so, then we would be dealing here with a characteristic phenomenon of the Internet Revolution. It would have strengthened the effect of profit growth policy that was pursued by

⁵¹ Data according to Bart van Tongeren, p. 6 of his unpublished review of the book *Onder uitgevers* by Vermeulen and De Wit, written in 2001 for the Society *Nonpareil*.

⁵² Interview of Guus Schippers, librarian of the Faculty of Physics and Astronomy at the University of Utrecht, on 13 September 2006: 'The subscriptions were terminated in 1999 and concerned 20 journals. These journals had high rankings in the citation index, but at most a quarter of these were regularly consulted, definitely less than the cheaper and very good journals of the Institute of Physics and the American Physical Society. Faculties in America had taken the step before, but the other Faculties in the Netherlands didn't follow and kept their Elsevier-subscriptions—despite the fact that ever more research workers were finding their way to the Public Library of Science on the Internet.'

all *STM* publishers. Profit was absolutely essential for Elsevier, which had been looking for a partner with capital since the end of the 1980s and only succeeded in doing so in 1992 (in Reed, a company already mentioned and which, although it had three times as many staff, was worth the same on the stock market)⁵³ in order to defend itself on ‘the battlefields of electronic access and the threat of takeovers’ as the director of Wiley put it. Reed Elsevier, which now included Butterworths, was able to acquire Harcourt in 2001, which also included Academic Press and Saunders. The details of these transactions are not relevant here. What is of relevance is that in 1996 disappointing profits led to the insight that science journals should increase less quickly in price and that their subscription should be coupled to cheap access to the electronic versions of these journals on the Internet.

And here we must take our leave. The Internet Revolution, which has radically changed science publishing, will require a separate book—a book that is at least as large as the one that we have written.

⁵³ Vermeulen & Wit pp. 172–178; Reed bought a certain amount of the Elsevier shares, by which the shareholder values of both companies became equal, at the ratio of 1 Elsevier share against 7 Reed shares; the fusion was announced in September 1992, and became effective in January 1993.

MANUSCRIPT SOURCES AND INTERVIEWS

Documents

Amsterdam, Bibliotheek van het Boekenvak (Library of the Book Trade, in loan at the University Library Amsterdam)

The project files and documents assembled and collected for this book have been deposited in this library

- (1) Letters February 2002–July 2005
- (2) Letters August 2005–May 2007
- (3) Notes of interviews and progress reports
- (4) Varia

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Archive of *STM*

Heemstede, private collection

Papers of Daan Frank

Important sources for the history in Chapter 4 are a typescript by Floris Bakels, and papers by Piet Bergmans, which have not been published and are therefore not taken up in the Bibliography. We refer to them by *Elsevier 100*, *Chronicle* and *Personal Recollections* (see notes 2 and 6 of the Preface).

Important sources for the history in Chapter 5 are papers by Daan Frank, which have not been published and are therefore not taken up in the Bibliography. We refer to them by *Autobiography*, *Memoir K*, *Fragments*, and *Memoir B* (see note 5 of the Preface).

List of people interviewed

Akert, Konrad (Zürich) 09–12–03

Atkins, Marc (Amsterdam) 16–08–05

Bolman, Peter (London) 01–07–03; 11–09–03

Brown, Gerry (Stony Brook, New York) 17–03–03

Bueren, Henk van (Linschoten, Utrecht) 18–02–03

Cahn, Robert (Cambridge) 01–03–04

Dekker, Marcel (New York) 18–03–03

Edelman, Henk (New York) 10–08–02; 14–03–03

Ellis, Charles (Paris) 04–05–04

Eysinga, Frans van (Den Haag) 07–11–03; 05–10–04; 26–06–06

Frank-van Westrienen, Anneke (Heemstede, Haarlem) 28–06–05; 02–08–06

Fredriksson, Einar (Amsterdam) 05–11–03

Graham, Gordon (Marlow, Bucks) 12–09–03

Haar, Otto ter (Utrecht) 23–06–02; 23–08–02; 11–04–03

Kampen, Nico van (Utrecht) 29–10–02

- Koops, Wim (Haren, Groningen) 28-04-04
Manten, Arie (Breukelen, Utrecht) 16-11-05
Meijer-Praxmarer, Yvonne (Abcoude, Utrecht) 25-03-03
Nijhoff Asser, Paul (Amsterdam) 01-08-03; 01-03-06
Radt, Stefan (Onnen, Groningen) 02-04-03
Remarque, Jacques (Utrecht) 01-05-03; 18-05-03
Saugman, Per (Faringdon, Oxfordshire) 02-03-04
Saur, Klaus (München) 04-02-04
Schilperoort, Rob (Leiden) 19-07-06
Schimmelpenninck, Alexander (Dordrecht) 15-01-04
Schippers, Guus (Utrecht) 13-09-06
Siegbahn, Kai (Uppsala) 14-11-03
Tongeren, Bart van (Amsterdam) 12-02-02; 07-03-02; 19-04-05
Trapman, Hans (Heemstede, Haarlem) 15-09-05
Vinken, Pierre (Aerdenhout, Haarlem) 01-10-04
Wimmers, Wim (Sterksel, Eindhoven) 11-03-03

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